THE
PROCEEDINGS
OF
THE PHYSICAL SOCIETY

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THE PHYSICAL SOCIETY

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THE INTERNATIONAL DECIMAL CLASSIFICATION OF EACH PAPER IS GIVEN ABOVE THE TITLE

THE RT. HON. LORD RAYLEIGH, M.A., Sc.D., D.Sc., LL.D., F.Inst.P., F.R.S.

President of the Physical Society, 1934 *to* 1936

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SOME REMINISCENCES OF SCIENTIFIC WORKERS OF THE PAST GENERATION, AND THEIR SURROUNDINGS

By LORD RAYLEIGH, Sc.D., D.Sc., LL.D., F.R.S.

Presidential address, delivered January 24, 1936

§ 1. INTRODUCTION

THE composition of a presidential address is a matter of increasing difficulty. The supply of material for general comments on the methods and point of view of physical science hardly keeps pace with the demand. And as to the subject-matter of the science, the writers of books small and large are so active that as soon as a subject becomes ripe for general discussion the field is pretty sure to have been occupied already.

Partly under a sense of these difficulties and partly under the urge of some of my official colleagues, I shall depart from precedent, and take the opportunity of telling you something of my recollections of eminent scientific men of a past generation, and the circumstances under which they worked.

There is a difference of opinion as to how far personal idiosyncrasies and details of private life are worthy of record when they have little direct bearing on results. Usually such details of the lives of scientific workers are but scantily preserved, and to judge by the frequency with which the few available details are quoted and re-quoted, one may guess that most people are eager for them, and wish that contemporaries had handed them down to a greater extent than they have done.

For my part, I think that the history of science is quite as much involved with the personalities of the men who have made it as is any other kind of history. The familiar process of digesting original memoirs into text-books, however necessary, can only be accomplished at the expense of some sacrifice of true historical perspective; and the limitation of knowledge and the narrowness of the basis on which generalization rests are too often lost sight of in the process. To know something of the personalities of the men of science themselves, the conditions of their lives, and the point of view from which they worked, may often be a useful corrective to this tendency. Those who have had the good fortune to be in personal contact with the great workers of the generation above them should not neglect to record what they think may possibly be valued by posterity. There is no doubt that this duty has

been too little regarded in the past. It is hard to realize when one is young that what we see to-day may be gone to-morrow and irretrievably forgotten the day after: and still harder to realize how eager posterity may be to know it.

§ 2. LORD KELVIN, O.M., 1824–1907

Lord Kelvin was an intimate friend of my Father's, and I have told something of their relations in my book *Life of Lord Rayleigh* (Arnold, 1924). However, as I had the privilege of knowing him intimately from almost my earliest recollection up to his death in 1907, some further reminiscences can be gleaned which did not find a place in that book.

Lord Kelvin's mental activity and vigour were at all times most remarkable. He never seemed to be tired, or disinclined to discuss any scientific subject that might come up, and was always ready to do calculations in any spare moment. These were done in a quarto note book, his famous "green book" which he carried for the purpose. He wore tail coats with large side pockets, rather like a gamekeeper's pockets, in one of which the book was kept. His work was done in a large and rather unformed, but perfectly unambiguous handwriting. If there was anything Lord Kelvin avoided, it was cloudiness of expression: and the legibility of his handwriting was, in my mind at least, associated with this trait. The "green book" was produced, and the calculations proceeded with, on what might seem most unsuitable occasions. He seemed to find distractions rather helpful than otherwise, and would bring his work for preference into the drawing-room where ladies were gossiping, rather than avail himself of a quiet room. He would join in their conversation at intervals. In the laboratory, while waiting for something to be got ready to show him, the green book was produced, and I have on occasion been pressed into service to look out logarithms for him, and read them out, while he entered them. To be as insensitive to interruption as Lord Kelvin is a very valuable gift, and an unusual one. He did not always realise that other people lacked it. I have heard that there was a saying in the natural philosophy department at Glasgow that "the Knight [Sir William Thomson] cometh, when no man can work". The work of calculation went forward during train journeys, or in a hansom cab. During the sittings of the Royal Commission on Arsenical Poisoning from Beer (1901), of which he was chairman, he would put his own questions to a witness, and then relapse into calculations while his colleagues were putting theirs: and I have even seen him similarly occupied when sitting on the platform during a not very inspiring presidential address to the British Association. Fountain pens were not then much in use, and I think everything in the book was written in pencil. The photograph, fig. 1, shows Lord Kelvin, probably in about his sixtieth year, with his book. Note the side pocket in which it was kept. Lord Kelvin was always carefully dressed, in well-cut and reasonably new clothes appropriate to the occasion, with immaculate linen. He told us that when he was an undergraduate at Cambridge no member of the University would have thought it consonant with his position in life to wear any other garb than frock coat and tall hat or academical dress when walking in the main streets of the town.
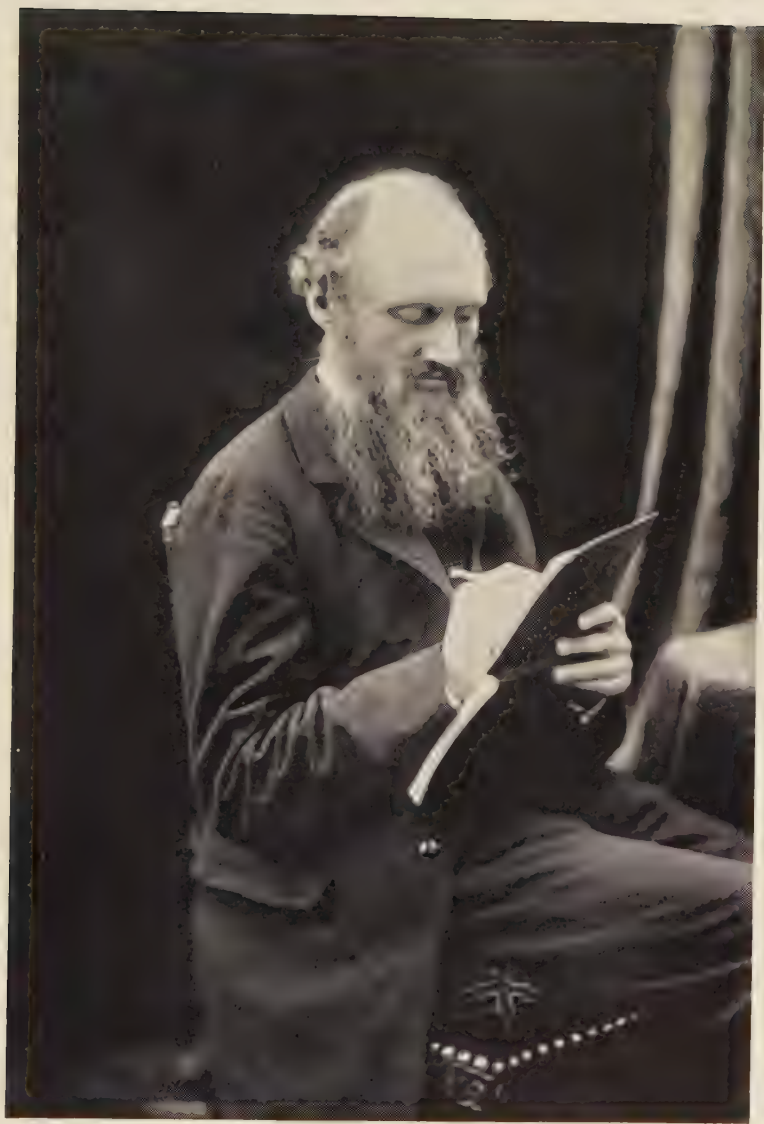
Figure 1. Lord Kelvin with his green book.

In the earlier days of his Glasgow Professorship, Lord Kelvin was a pioneer in encouraging students to work in his laboratory: but it cannot be said that he had anything amounting to what would now be called a school of experimental physics. Lord Rayleigh, for instance, about the year 1866, was anxious to get some initiation into the practice of experimenting, and found it very difficult to do so. Cambridge afforded practically no help, and he had never even heard of Glasgow as a possible alternative. Lord Kelvin's idea as to occupation for elementary students was not altogether the modern one. He thought that they should make themselves useful, chiefly in connection with the development of his commercial inventions. Sir William Ramsay used to say that he was set to work to take the kinks out of old copper wire! However, he eventually succeeded in getting hold of one of Lord Kelvin's electrometers, and made some measurements with it on electromotive forces.

One subject which Lord Kelvin was interested in discussing was the obscure phenomenon of globe lightning. He took up the point of view that such an effect could not be explained by or reconciled with the known laws of electrical conduction in closed circuits and that the accuracy of the recorded observations upon it must be denied. Sir George Darwin told me that on one occasion Kelvin had felt compelled to yield to some particularly detailed evidence on this matter from a competent witness, and for the moment frankly abandoned his old position. But, next time the subject came up, I do not know after what interval, Darwin was interested to observe that Kelvin had relapsed into his incredulous attitude. I think this psychological history is not an uncommon one. Belief in the marvellous involves a painful effort to a cautious and careful thinker, and unless the belief is constantly refreshed and maintained by a supply of new evidence, a relapse is very apt to follow. As to the truth of the phenomenon itself, reference may be made to a paper by Jensen[1] in which photographs are reproduced. The luminous objects shown on them are large, and by no means spherical.

Lord Kelvin's engineering methods were interesting and characteristic. He had probably never had practical familiarity with the use of tools; indeed it is not easy to visualize him as successfully driving home a screw: though one who, like myself, only knew him as an old man can hardly judge, and it is to be remarked that in his youth he was a successful oarsman, and played well on a wind instrument, both of which involve the appropriate kind of personal dexterity. His insight into the essentials of an engineering problem was, needless to say, very great, and his descriptions of his inventions such as his compass and his sounding machine are models of what such descriptions should be. They were composed with great care, and I have heard that in writing them, and in composition generally, he would sometimes pause for long periods before he could find the right word, or shape a sentence in the way that would satisfy him.

In designing his instruments and other mechanical constructions, he did not make use of machine drawing. His method was to get something actually made and then criticize it, and have it modified accordingly. This plan had its inconveniences. When long afterwards it was desired to copy Lord Kelvin's tide predicting machine, it was found that no drawings of the rather elaborate mechanism

had ever existed; and I believe that it had to be drawn out *de novo* from the existing machine. I believe, too, that his electrical instruments were made in the same way, being built up on a mahogany board from his verbal directions.

The view was sometimes taken, notably by Tait, that it was a prostitution of Kelvin's great abilities to devote himself to engineering and invention. It is doubtful, however, whether this view is really tenable. His brother, Professor James Thomson, cared mainly for engineering* and we may well suppose that Kelvin too had a strong natural bent in that direction. This is not inconsistent with his keen appreciation of the financial benefit of his inventions.

There was a certain competition for Kelvin's time and attention between Tait and Fleeming Jenkin. Kelvin came to stay with one or the other of them when he was in Edinburgh. With Tait he was writing the *Treatise on Natural Philosophy*. [Opinions differ as to who did the lion's share of this. Sir Alfred Ewing, from whose conversation (May 16th, 1932) I gained some of these details, thought that it was Tait. Lord Rayleigh, however, had the opposite impression.] Jenkin collaborated in Kelvin's inventions in connection with submarine telegraphy. He conducted all commercial correspondence connected with the very profitable royalties, and did this well, successfully avoiding litigation. Kelvin's compass inventions, on the other hand, involved him in a good many lawsuits.

Lord Kelvin's mind was not readily satisfied with any theory which did not lend itself to the formation of a mental picture. He required mechanical analogies, and it was perhaps partly for this reason that he was so much dissatisfied with the electromagnetic theory of light, which (in some moods at least) he did not consider could rank as a theory of light at all. His point of view was difficult to grasp: but one day when he was talking in this general sense, I ventured to remark that it seemed to me that at any rate it was a great step to have shown that the velocity of light could come out of electrical measurement. Lord Kelvin did not seem to admit that this remark was at all to the point when Maxwell's theory was under discussion. I asked whether it was not true that Maxwell had been the first to point out this relation and had he not done it in connexion with his theory? Lord Kelvin said no, it had first been pointed out by Kirchhoff, who was discussing conduction along wires. Needless to say that he was quite right, though I have only identified the passage with the help of Sir Joseph Larmor, while writing this address.†

The following anecdote, which I have from Professor George Forbes, who was an eye-witness, further illustrates his antipathy to this general line of thought. The

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* There was some personal resemblance between the brothers, and devoted affection. On the other hand, James Thomson was slow and cautious, in contrast with his brother's eagerness and enthusiasm, and would keep his scientific work unpublished for years. Lord Kelvin was often impatient with this.

† See G. Kirchhoff, "Über die Bewegung der Electricität in Drähten," *Ann. Phys.*, Lpz., **100**, 210 (1857), or *Gesammelte Abhandlungen*, p. 147. Maxwell's first mention of the relation appears to be in the B.A. Reports, 1863, and he does not mention Kirchhoff in this connexion either then or later. It is strange that so little should be known of this matter. It is not mentioned, so far as I know, in any British text book on electricity, and the few veterans who survive from the Maxwellian era seem for the most part to be unaware of it. There is a full account in G. Wiedemann's treatise, *Die Lehre von der Electricität*, **4**, 1076 (1881), but it is strange that when he comes to deal with Maxwell's results on p. 1178 he makes no further reference to Kirchhoff. A reader who consulted the latter passage alone would almost certainly conclude that Wiedemann knew nothing of Kirchhoff's work.

scene was at the British Association meeting at York in 1881, when Lord Kelvin was president of Section A, Silvanus Thompson wrote up some equations on the blackboard dealing with Maxwellian electro-magnetics[2, 3], which did not commend themselves to Lord Kelvin. For some reason, possibly with the idea that the president of the section should not speak controversially, he did not voice his objections, but as soon as S. P. T. had finished speaking, Lord Kelvin limped up to the blackboard and rubbed the equations out with a duster. S. P. T. took this in good part, and when Forbes reminded him of it some time later, he said that he had not given it a second thought. As is well known, it is to him that we are indebted for a masterly biography of Lord Kelvin.

Lord Kelvin had humour of a kind, but it was somewhat elementary, and he did not disdain puns, which, I suppose, were still fashionable when he was young. When he spoke of Hittorf's investigation on cathode rays, he said that he had " hit it off" very well. On this subject he was always very emphatic on the merits of Varley's short paper in the Royal Society's *Proceedings* in 1871[4]. Varley had shown that a light suspended vane was strongly repelled when placed in the path of the rays, anticipating Crookes' experiments on this matter. However, the forces brought into play in such experiments are much too large to be attributed to the direct effect of the bombardment, and this was pointed out in a paper by Starke[5], which I showed to Lord Kelvin. When I asked him next day what he thought of it he said he had not had time to read Starke's paper, but that he did not think it at all "strong"!

Rather more subtle was his remark to an electrical Journal, to the effect that one of his correspondents had better be submitted to the action of the " Silent Discharge ".

Lord Rutherford reminds me of the following incident, of which I am not altogether proud, but which ought to be recorded as showing Lord Kelvin's complete freedom from self-consciousness and his love of truth. It was in May 1904, when he and other scientific friends, Rutherford among them, were paying a visit to my parents. Lord Kelvin was enthusiastic in his interest in radioactivity, but much averse to Rutherford's views which regarded the energy as derived from atomic sources. Lord Kelvin considered at that time that the energy was drawn from the surroundings in defiance of the second law of thermodynamics, and he would sometimes say when discussing the matter with laymen, that he had hoped to go down to posterity as having formulated this law, and now he was compelled to admit that it was mistaken! He was arguing emphatically with Rutherford and myself against the atomic origin of the energy; so far as I remember he maintained that it was self-contradictory to appeal to atomic disintegration, the very definition of an atom, and the derivation of the word, implying that it did not disintegrate. I was enthusiastic for Rutherford's view, and with some lack of proper respect and deference, I asked Lord Kelvin if he would make a bet of five shillings with me that within three (or it may have been six) months he would admit that Rutherford was right. He seemed to be delighted with the proposal, and told the ladies about it, saying that he felt pleased that such confidence was placed in his good faith: for (he said) it was only by his own voluntary admission that he could possibly lose his money.

Within the allotted period Lord Kelvin's views had come round, and at the meeting of the British Association in the autumn of that year he made a public pronouncement in favour of the internal origin of the energy of radium[6]. I certainly never expected to hear any more about the bet: but next time I saw him, he came up to me at once and said: "I think I owe you five shillings. Here it is!"

But this was not the end of my troubles. I had become interested in the problem of the earth's heat, which, as Rutherford had suggested, might perhaps be explained by the radio-active matter contained in it. I made a series of determinations of the amount of radium in typical rocks, by getting them into solution, and boiling out the radon generated on standing. The result showed that there was enough radium even in the outer parts of the earth to account for the observed outflow of heat. I published a paper on the subject, but it soon got me into disgrace with Lord Kelvin. I received the following letter from him:

15, EATON PLACE, S.W.
June 11th, 1906.

Dear Robin,

I have been reading your last R.S. paper with extreme interest. Your experimental determinations of radium in rocks and meteorites are (it need not be said) exceedingly valuable and important.

But in your speculations on the "Earth's Internal Heat" you have apparently forgotten what you told me two years ago when you were showing to the R.S. what I called your perpetual motion;* that the energy of radium is all spent (? or half spent) in 1500 years. You have in your present paper reckoned on a *uniform* rate of expenditure of heat by radium in granite; the same rate as that of radium per gramme in bromide of radium. Surely on your own principles all the radium-energy of any mass of granite, large or small, and of any stony meteorite, would be all spent in the course of a few thousand years; and no granite or other mineral could now possess the unexhausted radium which you find in it. The fact that you do find it proves that radium in that combination *does not* generate heat at all, or generates it only at rates vastly less than the rate proved by experiment for separated radium bromide.

Yours,

KELVIN

I have not kept any copy of my reply but it may be reconstructed in outline from Lord Kelvin's next letter, which is given below:

15, EATON PLACE, S.W.
June 14th, 1906.

Dear Robin,

What you say in your letter of Tuesday does not give reason to believe that granite has been giving out heat for millions of years. I knew Soddy's work on production of radium from uranium, and I saw the beginning of his last experiment which is now being continued.

There is good reason I believe for regarding underground heat, and sun-heat as originally due to expenditure of gravitational energy in the coalition of the matter constituting earth and sun.

* This is the experiment which has become known as the "radium clock".

I am glad you are coming to London tomorrow. Will you and your wife come to tea or lunch some day soon? or will you come to breakfast 9.15 or 9.30 on Saturday. This would give us opportunity for a good talk over energy of gravity, and of radio-activity which I think is probably due to gravity.

Yours,

KELVIN

I accepted the invitation to breakfast, and afterwards we adjourned into his study, when he introduced me to his mathematical secretary, Mr George Green. "Not the George Green you know of", he said. His papers were neatly filed in rows of appropriate boxes—I remember one labelled "Income Tax". We soon got down to business. My paper was produced, and Lord Kelvin pointing to the conclusion, said emphatically that he did not believe for a moment that it could be true. I struggled for nearly two hours to get him to tell me exactly at what point he dissented, but without, as it seemed to me, any progress. After that, it was necessary for some reason to discontinue the sitting, but Lord Kelvin seemed anxious to resume it at the first opportunity. I was flattered, but a little puzzled by this, since he had not seemed to attach weight to anything I had to say.

I next saw him when he called unexpectedly a little later at my house at Cambridge. It was in the morning, and he was directed to the laboratory on the far side of the garden where I was at work. I heard my christian name called, and looking out to see who it was, there was Lord Kelvin, his white beard streaming in the wind. I went down and brought him up. His thoughts still seemed to be running on my heretical paper, and he was anxious to see my "rock broth", by which he meant the solutions of rocks used for the determination of radium by means of radon. He seemed disappointed when I had to tell him that it had been thrown away when the determinations were finished. However, I showed him whatever I was doing at the time, and as usual he was very interested and enthusiastic, as indeed he always was over experimental work. He took his leave, and I think that that was the last time I saw him. He died a few months afterwards.

However violently Lord Kelvin disagreed with people on scientific matters (and it was obvious that he did not mean half of what he said—if he had done so their views would not have been considered worth his powder and shot) he was always most careful not to say or write anything that would needlessly hurt their feelings. He was essentially courteous, and rightly expected to receive courtesy in return. On the occasion of the Jubilee of Sir George Stokes' Professorship at Cambridge, he got up among a surging crowd of people at an evening reception at the Fitzwilliam Museum and gave an *extempore* address on Stokes' scientific career. What he said was probably over the heads of the audience, even if they had been well able to hear it, which they were not. As might be expected under the circumstances they soon began talking, and Lord Kelvin did not conceal his anger.

Indeed he was not very apt at adjusting what he said to the capacity of his audience. He said the thing because it interested him, rather than because it was particularly likely to get "across the footlights". Very often this did not matter. His own interest in what he was saying was infectious, and kept his audience

pleased. Young ladies whom he met in society would be delighted to receive his confidence in this way. On the other hand, I remember Mr Joseph Chamberlain remarking that when scientific men were good enough to explain to him something of their problems and conclusions he could usually understand, but that Lord Kelvin left him completely mystified.

Lord Kelvin was a violent anti-Home Ruler, and it was probably this that brought him into contact with Chamberlain. It is curious to read[7] that his father, Professor James Thomson, had in childhood been a witness of the severities used in suppressing the Irish rebellion of 1798, and was led to sympathise with the rebels. It would be interesting if we knew fully the development of Lord Kelvin's views on this subject.

In his prime, Lord Kelvin cannot have been wanting in boldness of conception and faith in the possibility of success in heroic enterprises. His work in connection with the Atlantic cable is evidence of this. In old age he became more sceptical. He was curiously positive in his hostility to the conception of an aeroplane as a practical thing and he did not live quite long enough to see his views falsified. Motor cars he did see, but he did not like them, and was very emphatic in maintaining the wisdom and necessity of the early law which limited the speed to ten miles an hour.

I owe the following to Sir Hugh Shaw-Stewart. Mr Speaker Peel came on a visit to Ardgowan, at the mouth of the river Clyde. He had arrived by the night train from London, and had some little time on his hands before breakfast and before his host and fellow-guests had put in an appearance. He wandered out into the garden, and climbed to the ramparts of the old border tower, which was the original home of the Shaw-Stewart family, and which looks out over the sea towards the south. There he encountered a friendly old gentleman, not known to him, who was earlier than the other guests, and they fell into conversation. The old gentleman looked out to the south over the sea with a faraway dreamy look in his eyes and said, "It is strange to think that there is nothing between us and Spain!"

Mr Speaker Peel in fact thought it so strange that he was inclined to form an unfavourable opinion of the old gentleman's mental condition. They went in to breakfast, and the unknown was introduced as Sir William Thomson. Thereupon, Mr Speaker Peel thought that his first impressions might need to be reconsidered. He took an early opportunity of consulting the map, and soon found that the paradox was confirmed.

Lord Kelvin in his later years was somewhat deaf, and his desire for accuracy and precision made him unwilling to try to guess or reconstruct what had been said from imperfect indications, as most people are willing to do. This made conversation with him across the table rather exacting. He was fond of going to the theatre with Lady Kelvin, but when he did not hear, he asked her to repeat, and this happened so often that the occupants of the neighbouring seats were visibly annoyed. Lady Kelvin was ultimately forced to discourage theatre-going for this reason.

This brings me to say a word about the relations between them, which, at all events, so far as friends could judge, were ideal. They were entirely dependent on

one another. It was pretty obvious that Lady Kelvin did not know or care much more about science in the abstract than ladies in general do, but Lord Kelvin did not seem to be at all damped by this, and would unburden his mind to her at length about the most abstruse matters, while she placidly went on with her knitting. Professor Zeeman tells me that Lord Kelvin went into transports of delight when he showed him "the magnetic splitting of the spectrum lines", and was eager that Lady Kelvin should see it too. Lady Kelvin however did not make much pretence of sharing his enthusiasm.

It may be permissible to mention in passing a matter personal to myself. When I was engaged to be married in 1904, Lord Kelvin insisted on going with Lady Kelvin to select a wedding present; a pair of old silver cups were chosen, and they are among my treasured possessions. I have brought them to show you.

Lord Kelvin's last illness was aggravated, it was thought, by anxiety about Lady Kelvin's health, which was far from satisfactory. However, she made a good recovery, and survived him for a few years. But the light had gone out of her life, and she could not face the effort of going alone to houses where she had been accustomed to go with him.

These reminiscences have necessarily dealt in the main with Lord Kelvin as an old man, for it is only so that I knew him. Many of them, I am afraid, reveal some weaknesses that are incidental to that period of life, and those who knew him in his magnificent prime would have been able to draw a portrait worthier of the subject. Nevertheless there was much to admire in his old age, particularly from the human point of view. He was as enthusiastic as a boy, and entirely unspoilt by his success in life, and indeed apparently unconscious of it. To over-emphasize his own achievements or to dwell on them at all except in so far as they were interesting and relevant in themselves was a thing quite foreign to his point of view. The impression he generally produced on strangers was that of a dear old gentleman, and further acquaintance did not tend to change it. But with all his simple kindliness, his presence was on occasion most dignified and impressive, and the same may be said of Lady Kelvin, his devoted wife.

§ 3. ROYAL INSTITUTION

The old laboratories of the Royal Institution as they were towards the end of the last century, during the period of Dewar's large-scale liquefaction of the permanent gases, and Rayleigh's work on the isolation of argon, are but a memory. The rooms have for the most part been altered past recognition in the rebuilding operations and the men who worked there when I first knew the place have all passed away. It is worth while to try to reconstruct the scene in a verbal portrait; and I am encouraged to make the attempt by the circumstance that valuable contemporary photographs exist which were taken by Miss Reynolds and bequeathed to the Institution. Many of these have never been published, and I am allowed to reproduce a selection from them here. It is impossible altogether to avoid ground already covered in my *Life of Lord Rayleigh*, but I shall have much to say that is not to be found there.

In those days there was no "Director" of the Institution. Sir James Dewar,* who held the Fullerian Professorship of Chemistry, lived in the historic rooms on the top floor, which had been occupied by Faraday and Tyndall, and was "Superintendent of the House", exercising a general supervision over the building and staff. His rule was not supposed to extend to the physical laboratory, which was the domain of the Professor of Natural Philosophy (Lord Rayleigh).

Rayleigh had accepted the appointment in 1887 on the understanding that he would continue as before to work for the greater part of the year in his own laboratory at Terling, and that he would only be at the Institution during the season before Easter. Further, that the Institution would pay part of the salary of his assistant, George Gordon, who came up with him. During the season before Easter he gave a course of six afternoon lectures and a Friday evening discourse.

The physical laboratory, as it was left by his predecessor Tyndall, consisted of three rooms above the level of the street, but below the lecture room. They were situated immediately over the chemical laboratory in the basement, and the vibration from the machinery installed there by Dewar made many kinds of physical measurement impossible to carry out.† However, Rayleigh's season there was limited, and a good deal of his time was occupied in preparing for the afternoon lecture on Saturdays, and devising demonstration experiments, so that not very much systematic research work was attempted.

When Rayleigh came there he found the equipment very inadequate, and reported so to the Managers. There was no strict allocation of the available apparatus and supplies of material between the two laboratories, and, as may be imagined, considerable forbearance was needed to make such a system tolerable. I was about to say mutual forbearance, but the term hardly seems applicable, as the forbearance was nearly all on one side. Rayleigh and his assistant Gordon found an almost complete want of ordinary stores such as screws, copper wire and the like. They did their best to remedy the deficiency: but with little advantage, for their store was at once regarded as a plundering ground by the Scottish marauders from downstairs. Not being there continuously, they were not in a strong position for guarding these treasures and any attempt to lock them up was resisted as "contrary to the custom of the Institution". I believe Gordon was ultimately reduced to hiding these elementary necessaries.

Rayleigh was assigned a grant by the Managers to supply the most serious deficiencies of apparatus but here again he was met by the same difficulty. For instance, he felt the want of an electric motor for general purposes. He took steps to provide one and enjoyed the use of it for a time. But when he returned the next year he found, somewhat to his annoyance, that it had been permanently absorbed for driving a lathe downstairs.

As I have already explained, Rayleigh did not usually attempt anything very important at the Royal Institution laboratory apart from preparing his lecture

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* Knighted in 1904.

† It has been thought probable that this vibration set up the mischief which ultimately resulted in the collapse of the library ceiling.

Figure 2. The Physical Laboratory, Royal Institution. Apparatus for concentrating argon.
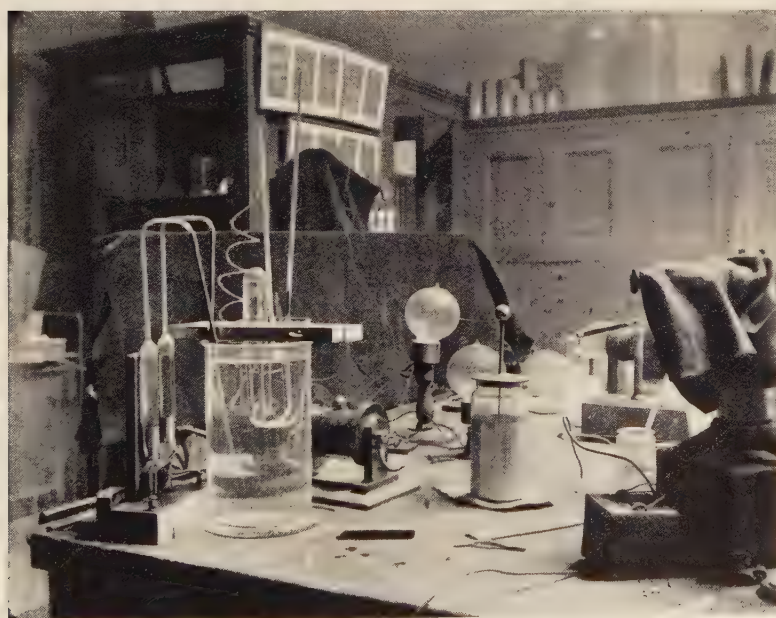
Figure 3. The same. Small-scale argon apparatus.

demonstrations. The chief exception to this was in the large-scale separation of argon from air. I say large-scale only relatively to the standards of that time, when 50 cubic centimetres was considered valuable. He had separated a quantity of that order of magnitude in his private laboratory at Terling by the process of sparking air with excess of oxygen, over caustic alkali, and the next step was to make a much larger supply, for the determination of its physical properties, more especially the density. His annual before-Easter season in London lent itself very well to dealing with this matter, since the alternating electric supply was available there, while at Terling it was necessary to run a gas engine continuously and even so the power was not enough.

About 3 litres of argon were required to fill the globe in use at Terling for weighing gases. This involved the absorption of 300 litres of nitrogen, or about 800 litres of the oxygen-nitrogen mixture. The vessel used was one of 20 litres capacity cooled by a water-jacket above, as may be seen on the right of the photograph (fig. 2). The Swinburne "hedgehog" transformers (open magnetic circuit) are seen to the left of it and then the aspirator bottles used for measuring the rate of absorption. The gas holder containing the main supply of mixture is at the extreme left.

The low-frequency note of the transformers was heard from morning till night during this period: and any occasional intermission left a sense of something wanting. Dewar, passing on the staircase and, as usual, humming a tune to himself, was struck by this, and came in remarking: "I don't hear your hum." "No," thought Gordon to himself, "but I hear yours."

The manipulation of the apparatus was not free from danger, and as a precaution Rayleigh made it a rule for himself and others to stand on an insulating mat and to *touch only one thing*. The rubber door mat was noticed by Mr Swinburne* when he came in to advise about the transformer, and he remarked that the "rubber" mat probably contained about 1 per cent (?) of indiarubber. (At that time the price of raw rubber was very high, and the quality of "rubber" goods, e.g. the rubber tubing used in laboratories, was deplorable.)

The operation went on for three weeks. It was debated whether it should be left going at night, but there was no one but one of the porters to attend to it, and "Richard"†, who apparently had no confidence in the man, said in his laconic way, "better not". Rayleigh, who was hesitating, was impressed by this, and contented himself with working during the hours when the scientific staff were there. The concentrate, not yet free from nitrogen, was removed to Terling for the final purification and weighing.

There is a still larger sparking globe of 50 litres capacity preserved at the Royal Institution as a relic. It is fitted with massive platinum electrodes, and was cooled internally with a fountain of alkali, instead of being water jacketted. This vessel was used in later experiments to develop the method, but its historic interest is less.‡

Fig. 3 shows another view of the physical laboratory. The most conspicuous

* Now Sir James Swinburne, Bart., F.R.S.
† See below, p. 236.
‡ The sparking vessel used at Terling, similar to that of figure 2 but smaller, is now at the Science Museum, along with the weighing globes and other relics of Lord Rayleigh's work.

object is the small sparking vessel of test tube form, standing over alkali, and the voltmeter used to feed in oxygen, or hydrogen for the removal of surplus oxygen. This was used for analyzing small samples of gas for their argon content.

The chemical laboratory of the Royal Institution was in the basement, below the level of the street. When I first saw it, probably about 1889, the most conspicuous object in it was a tube 18 metres long, which was filled with compressed oxygen for observations on the absorption spectrum. As his work on the liquefaction of gases progressed, and as the scale of operations increased, it gradually took on more and more the aspect of a factory, full of machinery run by means of shafting from a large gas engine.

The earlier method of liquefying air depended on compressing it at the temperature of liquid ethylene evaporating under the air pump. The ethylene was not commercially available, and used to be made in a glass carboy from alcohol and sulphuric acid. It was stored in a gas holder in the small well or yard outside the big window of the laboratory. This may be seen in fig. 4.

By this method Dewar liquefied air on a scale never before attained, and showed the properties of the liquid for the first time to large audiences.

Later about 1895 the Linde method of liquefying air by means of the Joule-Thomson effect came into use. It was not easy to realize that precooling by more easily liquefied gases could be dispensed with, and Dewar never gave up the idea of "cascade". He eventually discontinued the use of ethylene, but carbon dioxide was always retained. A large machine was ultimately built, capable of producing 20 litres of liquid air per hour, and this was preparatory to the liquefaction of hydrogen—the culminating triumph of Dewar's life. As is well known, this was done by passing the compressed gas, precooled to the temperature of liquid air boiling *in vacuo*, through a coil, and allowing it to expand through a valve at the bottom, with regenerative cooling. This was in 1898–9.

It was soon found that the great trouble in liquefying hydrogen was the blocking of the tubes by solidified air, or other impurity in the hydrogen. The hydrogen commercially obtainable in cylinders at that time was not good enough, and it was necessary to prepare it from zinc and sulphuric acid. The generator was in the yard outside and the hydrogen was stored under pressure in a row of tall steel cylinders each capable of holding 300 cubic feet. The gas was admitted at the bottom and drawn out for use at the top. It was necessary to go up a 12 ft. steel ladder to get to the exit valves. The cylinders, I was told, were made from disused big guns from Elswick.

When all this machinery had been installed, little ordinary laboratory accommodation was left, except the bench under the large sloping window, where a row of vacuum vessels may be seen hung up (fig. 4). Later, a room was added forming part of the house next door, which had been bought by Dr Ludwig Mond for the Davy-Faraday Laboratory. Dewar is shown in this room in the well-known photograph prefixed to his collected scientific papers and also to the Royal Society obituary.* It is now used as the workshop. Dewar must, I suppose, have really

* See reference (8) In selecting the photographs here reproduced, I have excluded any which can be found elsewhere.

Figure 4. The Royal Institution. Chemical Laboratory. Plant for liquefying gases.

Figure 5. R. N. Lennox in the Lecture Theatre.

been grateful to Mond for his magnificent gift to the Institution, but his tone scarcely suggested it, and in talking to Rayleigh he took up a singular attitude, apparently regarding this generous benefactor as a sort of suspect who was on his probation.

Having liquefied hydrogen on a large scale, Dewar proceeded to an attack on helium. Some account of his ideas on this matter will be found in his presidential address to the British Association at Belfast in 1902, when he described the procedure which ultimately succeeded in the hands of Kamerlingh Onnes, namely regenerative expansion with precooling by liquid hydrogen boiling in vacuum. I saw Dewar's apparatus in course of construction. The regenerative coil was contained in a vacuum glass. Outside this was a larger vacuum glass (see below, p. 234) to contain liquid hydrogen and outside this again a simple glass cylinder to contain liquid air. Two outer glass vessels were to afford thermal insulation. The liquid helium, had it appeared, could only have been examined through no less than seven separate glass walls, or fourteen surfaces.

The observation that helium was contained in the gas from the Bath Spring was made by Lord Rayleigh, who had known the town of Bath well from his youth up, and was aware that gas bubbled up from the hot spring. This gas had been classed as mainly nitrogen, and he had originally thought that it might really consist in the main of argon. This did not prove to be the case, but it did contain some argon and helium. Dewar, anxious to obtain helium for his attempts to liquefy that gas, decided on using the Bath Spring as a source. He got the consent of the Corporation of Bath and went to considerable expense to arrange for collecting the gas on a large scale, installing near the spring a gas holder for collecting it, and a compressor to put it into steel cylinders for transport to London. There it was treated for extraction of helium. The helium present was only one-thousandth part of the whole, and was necessarily expensive when produced in this way, by transporting the crude gas from a distance. However, any method of obtaining helium at that time would have been costly: no doubt that had been faced, and was not the most serious objection. The real trouble was that the helium from Bath gas contained a considerable proportion of neon, which could not readily be removed by the methods of purification adopted. In the opinion of Mr W. J. Green, who was associated with the later stages of this work, the blocking of the tubes and valves with solid neon was the principal obstacle which prevented the success of Dewar's attempts to liquefy helium, though at the time this blocking was attributed to ordinary impurities, which it was supposed had escaped removal in the preliminary processes. From this point of view, it seems a considerable misfortune for British science that helium was ever found in the gas at the Bath Spring. If Dewar had extracted his helium from monazite, as Kamerlingh Onnes did afterwards, he would probably have succeeded in the liquefaction. Nevertheless, his large-scale operations with the Bath gas may be considered the prototype of those modern ones which have led to the collection of helium in quantities adequate even for use in airships. It must have been an acute disappointment to Dewar that he was outstripped in the race to liquefy helium. But he took it with a good grace and himself gave an

exposition of Kamerlingh Onnes' work at the British Association meeting at Dublin in 1908.

This pioneer work on liquefaction of gases made large demands not only on the skill and persistence of the workers but also on their personal courage. On one occasion when I happened to be present I was struck with the iron nerve and composure of both professor and assistant. A, to me, alarming explosion rent the air of the laboratory, but Dewar did not move a muscle, or even turn to look. I asked in alarm what had happened; but it was only a good-sized vacuum flask full of liquid air which had smashed to atoms, in the hand of his assistant Lennox. The incident was not thought worthy of a word of comment except in answer to a visitor! Dewar never admitted that anything was dangerous. The most he would say was that it was a little tricky. Considering that Lennox and Heath, his two assistants, each lost an eye in the course of the work, this was certainly not an overstatement. On one occasion, at least, after a run for liquid hydrogen, a large leak developed, and the issuing hydrogen took fire, forming a sheet of flame right across the room, cutting off access to the stop valve at the top of the tall storage cylinder. Johnson, the mechanic, who was in the next room having his tea, realized what had happened, rushed in and sprinted up the ladder. The clicking of the rachet key which operated the valve at the top of the storage cylinder was a welcome sound.

I once met Sir Frederick Bramwell, who was then Secretary of the Royal Institution, in Dewar's rooms and heard him chaffing Dewar in a way that suggested that he thought his engineering practice highly unorthodox, proper factors of safety and speed limits being ignored. In the published accounts of Dewar's work on liquefaction of gases the engineering aspects are passed over very lightly, and there is a practically complete absence of dimensioned drawings such as would be really helpful to those who wished to follow in his footsteps. This contrasts with the practice of other workers in the same field, both before and since; and it contrasts also with the ample details given by Dewar himself about the vacuum vessels used for storing the liquids, and the various demonstration experiments on vapour pressures, and on solidification by evaporation *in vacuo*. The reason given was that the methods were not new and that "science derives no benefit from the description of transitional apparatus when there is no secret about the working process and how to carry it into effect". No doubt this is true up to a point, but as a matter of fact other workers were often eager to hear and see the details of Dewar's working methods, and were not allowed to do so. He disliked visitors coming unannounced into his laboratory, and on occasion would make a violent protest before he had adequately considered who the visitor was, and what right he had to be there. This more than once led to trouble and on one occasion at least Dewar apologized, saying that he had spoken hastily, but that if the visitor knew how much he had suffered from the piracy of his ideas, he would understand how it had happened.

The policy of secrecy was not altogether easy of execution. Those who came in to lecture at the Institution from outside had necessarily to have the help of the

laboratory staff in getting their lecture demonstrations arranged. The physical laboratory, whether Lord Rayleigh was working there or not, was open to them without reserve, and this made it rather difficult to carry out a different policy as regards the chemical laboratory downstairs which, from the standpoint of the members of the Institution and of the Board of Managers, was on a precisely similar footing. Indeed, I believe it is correct to say that the original rules of the Institution actually allowed the members the use of the laboratories though this rule was practically obsolete. They were, however, entitled to see what was going on, and one of them at least successfully asserted this right. Later, during the War, there was a movement among the members to insist on the laboratories being more fully utilized for war purposes and it was agreed by the Managers, apparently not much to Dewar's satisfaction, that a Committee from the Ministry of Munitions should visit the laboratory to see if they could make use of it. However, as one of them told me, they found everything covered with dust-sheets. The sheets themselves were clean, but there was a good deal of dust on the machines which they covered, and it was reasonably suspected that the sheets had been specially put on in honour of the visit. They retired discouraged, and the matter was dropped.

When, some time later, Kamerlingh Onnes was in England, he wrote asking to see Dewar's laboratory: but Dewar civilly begged to be excused showing it, saying that then no awkward questions could arise. It was difficult to see any adequate reason for all this mystery, and I think it was mainly temperamental. The usual reason for secrecy is in the commercial value of unpatented knowledge. But Dewar gave his invention of the vacuum flask freely to the world, and never made a penny by it, though he might have realized a fortune.

It is no secret, and indeed often appears in the present account that Dewar was quarrelsome. So far as I am aware those who were most conversant with the circumstances of the various quarrels generally considered him to be in the wrong. Nevertheless he was essentially kind-hearted, and if his tongue was rough, he was ready enough to respond to a friendly advance when he had cooled down, and, I suspect, attached little importance to what had passed, considering that hard words break no bones.

He was a bad sleeper, which explains and excuses much. I have heard it said that he was glad to get even three hours. He was something of an amateur pharmacologist, and had a special sleeping mixture of his own composition, which he took regularly, and sometimes recommended to his friends.

He was a generous man, and if anyone with whom he was associated needed help, he gave it unstintingly, and would not listen to a word of thanks.

His rooms on the top story, those which had formerly been occupied by Faraday, were a centre of hospitality after the Friday evening lecture. He was something of a collector of furniture. One of his treasures was a pair of bellows of Renaissance work carved with spirited figures, which he had bought (I think from Wertheimer) because, as he said, bellows were "the chemist's first implement". When these were cleaned and closely examined he found traces of a signature, with place and date,

which led him to attribute them to Benvenuto Cellini. Whether this attribution was accepted by experts I do not know.

Dewar's training had been primarily that of a chemist. His earliest investigations were on organic chemistry, but in addition he was a man of wide knowledge and essentially original scientific outlook. It was difficult to get much insight into his mental processes by talking to him, or even by listening to his lectures. He had no notion of unfolding his views in logical order, or of clearly explaining by what train of thought he had arrived at them. But however mysterious its origin there was no doubt of the value of his inspiration. The invention of vacuum-jacketted vessels naturally occurs to every one: and if they had come to be popularly called Dewar flasks instead of thermos flasks, his fame would doubtless have penetrated to a very wide public.*

The use of charcoal cooled in liquid air for producing high vacua and for isolating helium was an invaluable laboratory method, and contributed much to the success of the earlier researches on positive rays, and on the rate of generation of helium from radio-active bodies. Its value is now diminished owing to the modern invention of other methods of rapidly producing high vacua, but is still by no means negligible.

Then again, men have watched the beauties of soap bubbles from time immemorial, but it was Dewar who first penetrated the secret hidden from all other men and succeeded in making them permanent by the use of pure dust-free air. It can hardly have been anything but a happy guess that led him to this: but a guess that he alone had made and verified.

Having now said something about the leaders and their work, I wish to notice the subordinates. Rayleigh's assistant, George Gordon, who was with him alike at the Cavendish Laboratory, at Terling, and at the Royal Institution, I have already noticed in my *Life of Lord Rayleigh*. Heath, the lecture assistant, was civil and helpful, but not in any way outstanding.

Dewar's chief assistant, Robert Lennox, who was with him from 1881 to 1908, was an interesting personality, and his relations with his chief formed a curious psychological study.

My information about his life-history is fragmentary, but he must have been still quite young (not more than twenty-three) when he went to the Royal Institution. He told me that he had had a training in engineering science under Professor James Thomson at Glasgow, which had enabled him to design and make working drawings of the machinery used by Dewar in the liquefaction of gases. He was also at one time chemical assistant to W. Dittmar at the Glasgow Technical College. He does not seem to have taken a university degree, but his general scientific capacity was far beyond what is usually found in non-graduates, or for that matter, in graduates either. His services were worth more than the very limited funds of the Royal Institution could afford, and Dewar supplemented Lennox's Royal In-

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* Dewar's earliest publication of the idea of a vacuum vessel dates back as far as 1874, long before his successful use of it for liquid air in 1893. There is no justification for any suggestion that he had been anticipated in the matter.

stitution salary from his own resources. Fig. 5 represents Lennox in the lecture theatre, and I am glad to be able to reproduce it as a memorial to one whose services to science have not been sufficiently recorded.*

Dewar was always careful to acknowledge the value of the help that Lennox had given him, and expressed himself as follows (1899):

"During the whole course of the low temperature work carried out at the Royal Institution, the invaluable aid of Mr Robert Lennox has been at my disposal, and it is not too much to say that but for his engineering skill, manipulative ability, and loyal perseverance, the present successful issue [liquefaction of hydrogen] might have been indefinitely delayed."

This appears to me to be a fair and true general statement of the case. In some quarters not friendly to Dewar it was suggested that Lennox was the mainspring of the whole enterprise. This I have always considered to be nonsense. Dewar had been interested in the liquefaction of gases before Lennox joined him. Great initiative was required to lay out the laboratory for this class of work, to decide on the general line of attack, and to secure the necessary funds.† It is fairly obvious that no one but a principal could really do this. Moreover, in talking to Dewar, and reading his writings, one could not fail to discover that he was a master of manipulative detail, and very fertile of resource. [I have gained invaluable hints from him in this way myself, coupled with a characteristic warning that I was not to repeat what he had told me to ——, though I might tell my Father.]

Notwithstanding what has been said above, Lennox's share was very important. He, it is believed, was mainly if not wholly responsible for the design of the compressors and liquefiers in their engineering aspect. By a rather curious arrangement they were made by a firm, Lennox, Reynolds and Fyfe of which he was the leading spirit. Their workshop was at Chiswick on the Thames, and Lennox himself lived there. But how he was able to find time for this while he was in regular attendance at the Royal Institution it is hard to understand. He must have relinquished all leisure and recreation. On one occasion he invited me to see the Oxford and Cambridge boat-race from his premises, but unfortunately I did not accept the invitation, and missed the opportunity of seeing his methods of work there, which, if I dare confess it, would have interested me very much more than the boat-race. Lennox's engineering enterprises were apparently not at first very successful from a financial point of view, and financial reconstruction had to be resorted to more than once. It is probable that his duties at the Royal Institution prevented his giving adequate attention to the business. The large air pumps used in Dewar's laboratory for evaporating liquid air and liquid hydrogen *in vacuo* were of his design and construction. They were of the rotatory type with an eccentric barrel and sliding feathers,

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* A further idea of Lennox's personal appearance may be gained from the picture by H. Jamyn Brooks at the Royal Institution, representing one of Dewar's lectures on liquid hydrogen (1904). His is the bearded figure at the back partly turned away from the spectator, and handling an electroscope; No. 141 on the key plate, though his name is not given there.

† In the early days the cost of liquefying hydrogen was estimated at £20 for material each time. But of course the expense was mainly in installing the plant. In those days the whole idea of scientific equipment costing thousands of pounds was unfamiliar (except perhaps in connexion with astronomy) and such funds were not easy to raise.

and so far as I know they were the first successful air pumps of this design, though the fundamental idea as applied to water pumps is a very old one.* Improved air pumps of this kind have since been introduced by Gaede and others.

The personal relations between Dewar and Lennox struck me as very strange. Lennox admired his chief and, so far as I know, his way of referring to him was always respectful and decorous. But behind the scenes it was very different, and plain speaking on both sides was the order of the day. Indeed, I have been in the laboratory while delicate manipulations were in progress and whenever Dewar made a suggestion as to how some particular operation should be carried out, Lennox apparently made it a point of honour to contradict him, and to do something different. So far as I could observe this *riposte* did not generate any ill feeling, the parties understanding one another well, and finding it rather stimulating than otherwise.

On other occasions I believe it went further, and really high words passed. It was suspected by a favourably placed onlooker that if Lennox considered that too much had been said, he saw to it that the machinery should not be in working order when liquid hydrogen was next wanted. Many such quarrels subsided, each party really having a great respect for the other, and each being unwilling to bring the fruitful co-operation to an end. Finally, however, in 1908 the end came. It appears that an expensive plant designed and made by Lennox for the separation of helium from the Bath gas by rectification failed almost completely. Dewar complained in no measured terms, and Lennox as usual made his reply. If they had limited themselves to words the breach might have been healed: but they started exchanging angry letters, and a permanent quarrel resulted. Both regretted this when it was too late, but neither would make any advance, and Lennox never came near the Royal Institution again until after Dewar's death in 1923. Then he took an early opportunity of resuming the connexion, of which he had been very proud, by becoming a member.

Lennox's skill in laboratory arts was great, and he had that courage and confidence in his own ability to carry out any operation which goes so far towards assuring success. The early vacuum vessels devised by Dewar were made by him without any special appliances beyond an ordinary laboratory blowpipe and his two hands. Some of these historic vessels may be seen at the Royal Institution. He had a great belief in the importance of being in good form for such work, and when he felt himself to be so, no difficulty would daunt him. I have seen an enormous tubular vacuum vessel part of the helium liquefier, which, I was told, was made by Lennox using an oxygen blowpipe, with three men handling the glass under his directions. The manufacture of such things had then not been commercially organized and the machines which now make it comparatively easy did not then exist.

Lennox was gifted with a somewhat cynical sense of humour. Thus, he was preparing hydrogen for use in a hydrogen thermometer to determine the boiling point of liquid hydrogen and some criticism was made as to a possible presence of air in

* See reference (9). Young makes reference to Ramelli, who wrote as early as 1588.

the gas. "Well," said Lennox, "never mind that. It will make the temperature come all the lower."

Again, he was interested in an acid-resisting iron alloy. The peculiarities of this had apparently been noticed by a workman, who remarked in surprise "t'aint iron". From this incident the trade name of Tantiron was adopted. "It is curious", said Lennox, "that so many analysts should find tantalum in it."

Lennox was not fortunate in some of the happenings of his life, but his character was stoical in the extreme, and no blow of fate seemed to have any effect on his iron composure. A serious accident occurred when he was trying the generation of acetylene under pressure, with a view to storing it liquid in cylinders. There was a violent explosion. Sir Joseph Petavel was in the next room. The first thing he heard after the explosion itself was Lennox's voice saying "Look out, you men, put out the gas in there." Next, he saw Lennox, his face streaming with blood and one of his eyes was torn to pieces and the fragments spread all over his face, but quite calm. His first thought had been to prevent any further accident from the escaping acetylene. He always seemed quite indifferent to personal danger, and remarked that his old schoolmaster had been accustomed to say that a man born to be hanged would not come to any other end.

Again, in 1926, he came to see Sir Joseph Petavel at the National Physical Laboratory. He said that the doctors had given him three months to live and that they were probably right. Petavel tried to say something to raise his spirits, but he brushed this aside, saying that everyone had to die, and showing no signs of being in any way moved at the prospect. The doctors proved to be right, almost to the day.

Under Lennox was Robert Johnson, who went to the Institution in 1892. Dewar used to refer to him as "my old gasfitter", but he was in fact a competent mechanic, with considerable powers of leadership, great personal strength, and skill in making the best use of it. With the help of three or four men he would take down one of the compressors, repack the pistons, and have it ready for use again in less than an hour. He had a modest pride in his work; I remember, for example, cross-examining him about his methods of making soldered joints in pipes, and in reply to something I said about the difficulties of getting them tight, he remarked that there had not been any complaint of the joints since he had been there. Though he cannot have understood much of the scientific aspects of the work, yet he entered into the spirit of it as far as he was able, and expressed to me his pleasure in the liquefaction of hydrogen. "It comes out so clear", he said.

Like Lennox, Johnson was absolutely unsparing of himself, and it was hardly realized until his course was nearly run out that anything was amiss with him. One day he was working on one of the machines with a spanner, when Dewar noticed a lesion on his arm, and, always kind and solicitous as he was about matters of health, sent him off at once to a consultant physician to be examined, though apparently without fully realizing how serious the trouble was. Johnson put on his pilot jacket, and set off along Albemarle Street, with his square thickset figure and sturdy rather groom-like walk. It was for the last time. In ten days he was dead of "galloping consumption".

A bearded figure, well known to all habitués of the Royal Institution, is seen on the lecturer's right in Jamyn Brooks's picture,* ready to hand the pointer for wall diagrams and to switch off the lights for any demonstration requiring darkness. He was universally known as "Richard". No one at the R.I. ever used any other name—I find, however, on enquiry that his patronymic was Fowler. By training, he was a carpenter, and a good one. He came from Yorkshire, and had the independence of character which is usually associated with that part of the country. His politics were conservative and if he was moved to make a remark on subjects of public interest, it would often be to express satisfaction at a conservative victory at a by-election, or something of the kind. He had been under Tyndall, who, he told me, was an exacting master. It was amusing to see his relations with Dewar. Richard had commonsense backed by a good deal of quiet obstinacy, and if he had a point of his own he would stick to it with a minimum of words. If Dewar spoke sharply to him, he made it very obvious that he was not impressed. His capacity to stand up to so formidable a personality made him somewhat of a hero in the eyes of the younger members of the staff.

One of "Richard's" functions on Friday evenings was to make his appearance at the door when the clock struck nine, to summon the board of managers and the lecturer to enter. Rather an elaborate ritual was observed on these occasions, the lecturer entering closely attended by Dewar. Sir Charles Wheatstone, it used to be related, when about to give a Friday Evening Discourse, found his nerve fail him at the last moment, and bolted. Faraday, though, of course, quite unprepared, saved the situation by giving an impromptu lecture. I believe it was that afterwards printed as "Thoughts on Ray Vibrations"[10]†. Dewar often referred to this incident, and told the lecturer of the evening that it would not be allowed to happen again.

Another vignette is at the time of Queen Victoria's Diamond Jubliee (1897). Everyone put up some kind of illumination on their houses, but the Royal Institution (where the electric arc was originally discovered by Davy) had something in character, by putting up an enormous naked arc on the roof. It was struck by a supplementary carbon rod, and hand-tended, and it took so large a current as to cause a serious fall of voltage in all the neighbouring houses. It was conspicuous all down Albemarle Street, and I have the mental image of a drunken man in the street, staring up at it, and shouting "put out that light".

§4. SIR WILLIAM CROOKES, O.M., 1832–1919

Sir William Crookes‡ was a scientific worker typical of an epoch that has passed away. He was, I think, essentially of the school of Faraday. Like Faraday, he was of somewhat humble origin, and largely self-taught. Like Faraday he was skilful in experimental technique, and relied mainly on his own intuition rather than on any

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* No. 139 on the key plate.

† Faraday says, p. 349: "I do not think I should have allowed these notions to have escaped from me, had I not been led unawares and without previous consideration, by the circumstances of the evening on which I had to appear suddenly, and occupy the place of another."

‡ I call him so throughout, but his knighthood dated from 1897.

very definite reasoning to guide him as to the subjects to be taken up: and like Faraday he was without the measure of mathematical training that is now expected from every serious student of physics or physical chemistry. When he was in his prime, an equipment of this kind was by no means inadequate, as Crookes's own career very clearly proves; and it may be suspected that even now, too keen a critical faculty, and too much knowledge of what has been done by others, is not always an unmixed advantage to one who aspires to be a pioneer in new fields.

It was a favourite remark of Lord Kelvin that Crookes had started more absolutely new hares than any other man among his scientific contemporaries. One of the most notable, the radiometer, made its appearance at a time when scientific sensations were not so frequent as they have since become, and it excited a widespread interest. Radiometers were to be seen in the shop windows of chemists and opticians all over London, and in provincial cities as well. They were indeed calculated to excite the curiosity of almost anyone as they spun round vigorously in the sunlight. The chief scientific interest was aroused at first from the idea that the effect was directly due to the pressure of light, the existence of which had for long been thought probable. If this were so, the mechanical reaction of the pressure would be on the sun. When Schuster showed that the reaction was in fact on the glass bulb of the instrument much of the sensational interest evaporated. These events occurred about the time I was born: but as a boy I well remember the radiometer to be seen everywhere in the shop windows. The revolving vane was of course a toy, and of little use as a measuring instrument, but torsion radiometers have been developed as delicate detectors of radiant energy by E. F. Nichols and others.

My first visit to Crookes's house was in 1896, in company with my Father; Crookes had seen the early experiments on the isolation of argon, by removing the nitrogen from air by sparking with an induction coil, actuated by a primary battery and hammer break. He invited my Father to come and see his own electrical equipment, which he had found to be capable of oxidizing nitrogen with comparative rapidity, and kindly allowed me to come too.*

We were received by Crookes and his wife. Lady Crookes's interests were entirely of a domestic nature. She was wrapped up in her husband and family, and never tired of telling of their doings. She delighted to help him when she could, and, she told me, often carried out the weighings for him in his chemical work, work which, as she said, was suited to the delicate fingers of ladies. It was obvious how devoted Crookes and she were to one another.

We had lunch in the dining-room on the ground floor, where there was a full-length portrait of Crookes as a considerably younger man, with a golden yellow beard, standing beside a table on which was an induction coil, and holding a pocket spectroscope in his hand. By an odd freak of memory I recall a chocolate pudding which was so popular that there was not enough of it to go round.

During lunch our host told of a recent incident in connexion with the water analyses which he carried out in collaboration with Dewar for the various London

* His house was No. 7, Kensington Park Gardens, not far from Paddington Station. This was not the scene of all his best known researches. He had moved there in 1880.

water supply companies of those days. The analyses from the supplies which came into London from different directions showed a remarkable similarity. Suspicion was aroused. The man whose duty it was to take the samples was "shadowed" and it was found that he filled up all the sample bottles from the same tap! As a natural result he was, as Crookes expressed it, "presented with the sack".

After lunch we went up to the library on the first floor. This was the best room in the house. There was a drawing-room for Lady Crookes to receive her friends, but it was on the ground floor, and I never penetrated to it. The library was lined with books, and there were various objects of scientific interest distributed about as ornaments: for instance a cut slab of meteoric iron; a shield-shaped piece of Labrador spar over the fireplace; and the largest radiometer I have ever seen. There were various mahogany cabinets containing specimens and under a glass shade an apparatus which had been used by Faraday in supporting specimens to be tested for dia- or para-magnetism. This, he told us, he had brought away from the Royal Institution when he had gone there to give a lecture. I think he had actually seen Faraday using it. The room was comfortable and well furnished, and everything in it was scrupulously tidy. No papers were lying about. They were all carefully filed: and there were neat box files on the shelves containing off-prints of scientific papers. I have never seen a more orderly man than Crookes. He never scribbled memoranda, or labels, and he never put anything away except in its proper place. There was an entire absence of litter in his library or his laboratory, and this result was not, as in some other cases, a symptom of inactivity.

Crookes did all of his correspondence, which was extensive, at a typewriter (fig. 6). He must, I imagine, have learnt to use it with facility late on in life, for typewriters can hardly have been available when he was young.

We went into the laboratory which consisted of a suite of rooms on the same floor as the library. There was a physical laboratory (fig. 7), which had the same general air of tidiness as the library, with glass cases containing many radiometers and vacuum discharge tubes. Seeing me examining these, Lady Crookes remarked: "We have not nearly so many of them as we had. The ex-emperor Pedro of Brazil came to see us, and asked for many of the best ones, and Mr Crookes did not see how he could refuse." I told this afterwards to my Father—and he said he thought that he personally would have refused!

The physical laboratory was rather restricted for space, and such space as there was was largely occupied with fixtures, so that it would to all appearances have been difficult to find room for working on any other class of problem than the high vacuum and electric discharge work which Crookes had made his own.

Crookes had almost inevitably been concerned in the development of the electric lamp industry, and his assistant Gimingham who had helped him in his well-known researches on the radiometer and on "radiant matter" (kathode rays) and who had acquired skill in glassblowing and vacuum technique, afterwards went into the young electric lamp industry, and no doubt assisted materially in its birth.*

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* The glass globes used by Rayleigh in his weighing of gases, which led later to the discovery of argon, were made under Gimingham's supervision.
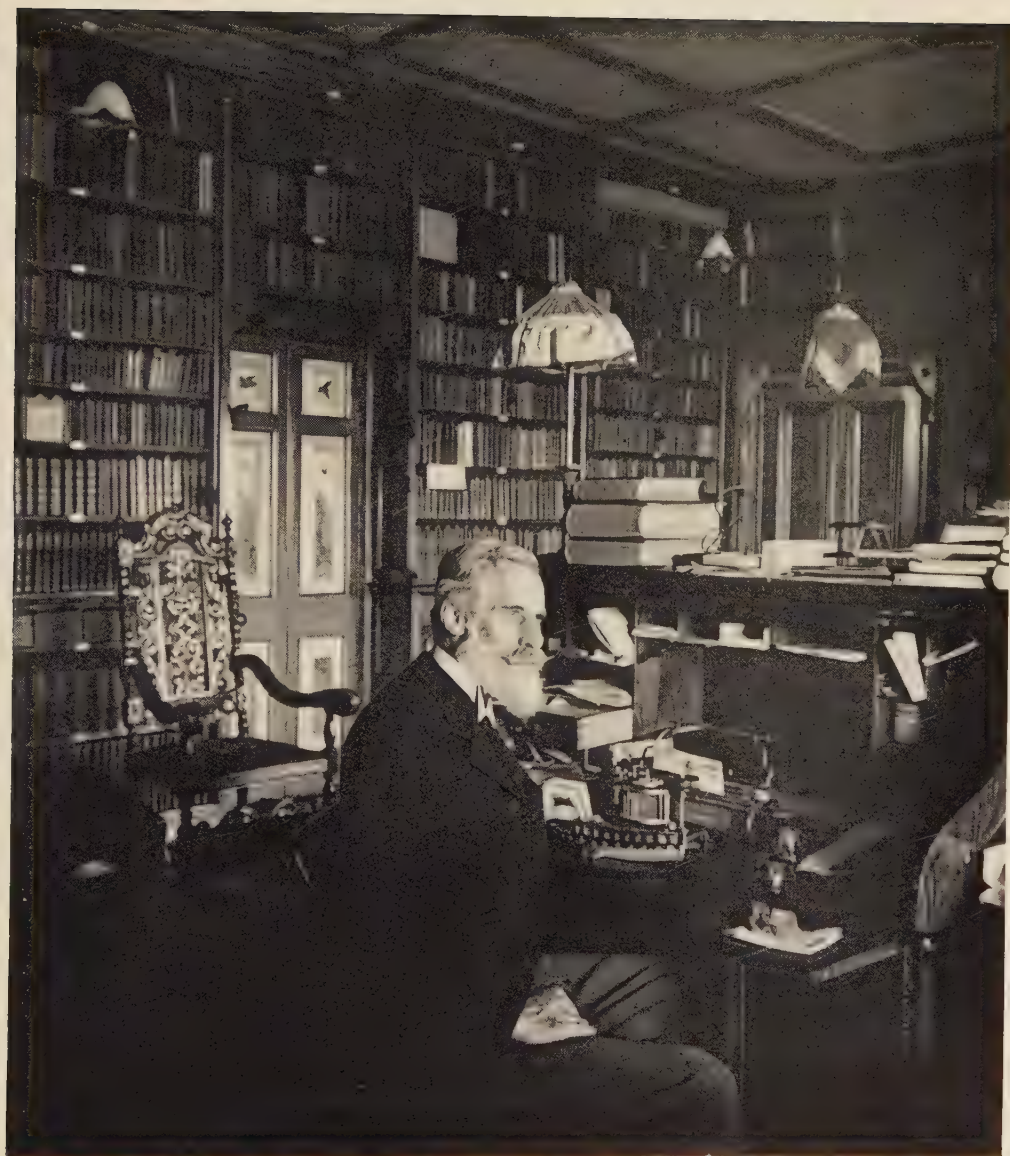
Figure 6. Sir William Crookes in his library.

Crookes showed us some of his work, including an electric lamp bulb which he described as the smallest in the world: I am not sure however that it would have impressed the rising generation so much as it did us. They might remark that they could buy torch lamps as small or smaller for a few pence at Woolworth's Stores! I also noticed, on the wall of the laboratory, what one might regard as the parent of the modern neon signs—a vacuum tube bent to spell the word "electricity". Needless to say, it was not charged with neon, which was only discovered some years later. Crookes had been one of the first to light his house electrically, and I have read somewhere that the wires were carried under the floor, insulated in glass tubes.* I learn from Mr Gardiner that the lamps were run direct from a dynamo driven by a gas engine, the plant being situated in a cellar dug out from the space on one side of the front door.

One of his reminiscences about the early development of lamp making was that women workers had been introduced to do the glassblowing, which in those days was done entirely by hand, the bulbs being blown from tubing. The girls at first wore their hair in an elaborately curled fringe across the forehead, in accordance with the prevailing fashion. However, if one of them ceased for a moment to work the foot bellows while her head was bent over the blow-pipe, the loose smoky flame rose up, with disastrous results to the fringe. As may be supposed, fringes did not continue to be worn in the factory for very long under these conditions.

The primary object of our visit was for our host to show us the oxidation of nitrogen, using his own plant which consisted of a motor-alternator, placed underneath the window frame so as to have the direct support of the wall, and a large induction coil of Apps' make, which he had used for his researches on high vacua, and which (I believe) is now in the Science Museum. This was used as a transformer. The arc discharge itself was surrounded by a loose flame.†

Crookes had it enclosed in a glass globe, in which the red colour of nitrous fumes soon became apparent. When the globe stood over alkali, absorption occurred almost immediately.

Crookes showed us casually an interesting experiment which, by way of a diversion, I will repeat for you now. Glass rods are attached to the terminals of the induction coil, and, if I warm them, to make them conducting, you see that the flaming discharge passes as well from these glass terminals as from metal ones.

Beyond the main (physical) laboratory were other rooms. One of them was a small workshop, containing a Pittler lathe, and a small shaping machine, blow-pipes for glass working, and so on. The other was a chemical laboratory, where had been carried out Crookes's elaborate fractionations of the rare earths. His work in this

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* The first electric installation I personally saw was that put up by the late Lord Salisbury at Hatfield House. Round the gallery of the library, simple cotton-covered insulated wires were carried in full view. A man had (somewhat inexplicably as it now appears), been killed by accidentally touching a live wire at a moderate voltage during the erection of the plant. I was about seven years old and a young girl cousin and I toyed deliciously with our fears by touching the cotton-covered wires. As the current was not on, the danger was perhaps not so great as we supposed. This was in 1882.

† Many years ago I showed that this "flame" is identical with the afterglow observed in vacuum discharges. It is possible to pass continuously from the former to the latter. The flame, like the afterglow, is due to the oxidation of nitric oxide by ozone. See reference 11).

direction must have involved immense labour, and he told me a little after this time that, though he hoped to glean a few more facts, he did not expect to see anything approaching finality. Indeed, it is doubtful whether these problems could ever have been satisfactorily resolved without the help given by X-ray spectra, which only became available much later. Crookes was at one time inclined to the view that in the rare earth group there were numerous elements differing from one another almost insensibly; and his speculations on this subject could plausibly be represented as heralding the modern conception of isotopes. However, as time has shown, they had not in reality a sound experimental basis.

To me personally, as boy and man, Crookes was most kind and friendly. Knowing me to be keenly interested in vacuum tube work, he asked his assistant, Mr Gardiner, to make a cathode ray tube for my instruction. Some yttria was treated with strong sulphuric acid and ignited. It was placed in the discharge tube, and highly evacuated with the Sprengel pump. Looking back over the years with some experience of such matters gained in the meantime, the whole technique seems to me to have been most efficient, and carried out in a wonderfully short space of time. No doubt the appliances then available were poor, judged by modern standards, but it was striking to see what they could do in skilled hands. The yttria under cathode ray bombardment shows the characteristic "citron band" spectrum which Crookes had discovered, and which enabled him to prove the presence of yttrium in all kinds of calcareous materials. I have the tube still, and have brought it to show you.

It was a source of great annoyance to Crookes that he had missed the discovery of the X-rays. According to the account he gave in my hearing, he had definitely found previously unopened boxes of plates in his laboratory to be fogged for no assignable reason, and, acting I suppose in accordance with the usual human instinct of blaming someone else when things go wrong, he complained to the makers, who naturally had no satisfactory explanation to offer. I believe it was only after Röntgen's discovery that he connected this with the use of highly exhausted vacuum tubes in the neighbourhood. He had, at least, less to reproach himself for than another English man of science, who, it was said, knew that high vacuum discharge tubes were apt to fog photographic plates anywhere near them, and only drew the moral that the plates should be stored elsewhere.

Crookes' first visit to South Africa was in 1895, just after the Jameson Raid. He had gone out to give evidence in a patent litigation. After the South African War, he and the late Lord Haldane were appointed to sit with my Father on the Explosives Committee of the War Office, designed to make good some of the deficiencies which the war had revealed. They came to Terling to discuss preliminaries with him. This was in June 1900. My Father did not like the smell of smoke, but Haldane was an inveterate smoker of cigars, and as deputy host I sat up with him and Crookes in the smoking-room. The conversation turned on South Africa, and Crookes spoke of his visit to Pretoria, and his interview with President Kruger on the latter's verandah, or stoep, decorated with the well-known stone lions. One of the things that seemed to have impressed him strongly was the disreputable ap-
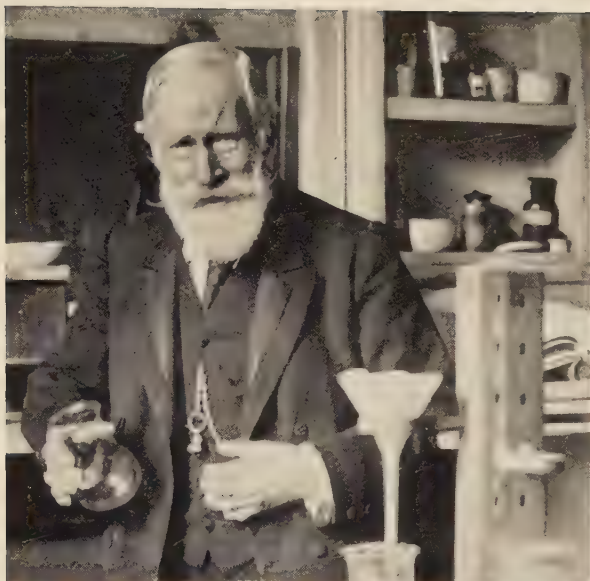
Figure 8. Sir William Crookes in his chemical laboratory.
(Near the end of his life)

Figure 7. Sir William Crookes's physical laboratory.

pearance of the Presidential tall hat, which he said was worse than anything he could have imagined. On the question of the Uitlanders and their political rights Kruger represented that all difficulties between them and the Boers would settle themselves in the course of a generation by inter-marriage. This seemed plausible enough at first hearing, but, on learning more of the subject from other sources, he found that all that the President had told him was "a pack of lies". It must be remembered that this conversation was held too soon after the war for a very tolerant or judicial attitude to be possible.

Another of Crookes's anecdotes shows him as a man of action. He had had a difference with a business partner, and the partner refused to give up certain documents to which Crookes laid claim. These, he had reason to believe, were in a city office [I suppose belonging to the partners jointly] of which the other partner was in possession. Having sought legal advice, Crookes proceeded to the office in the early hours before anyone was about, taking a locksmith with him. The lock was forced, and the documents recovered.

Crookes was deeply interested in the properties of the diamond, and in the peculiarities of individual diamonds. He was in touch with the De Beers Company, and I well remember the contempt with which he spoke of the action of "our precious government" in driving the company's headquarters from London to South Africa, as a result of income tax regulations. He had early observed the brilliant phosphorescence of some unusual diamonds under cathode rays, or "radiant matter" in his own phrase. Later, he repeated the experiments of Moissan on the artificial production of diamonds in the electric furnace, and considered that he had confirmed the results. However, after Crookes's death, the hard crystals which he had obtained came into the hands of Sir Charles Parsons, who was unable to induce them to burn in oxygen, and concluded that Crookes's identification of them as diamonds was mistaken.

Crookes experimented too, on the coloration of diamonds by radium rays. He produced some beautiful specimens of coloured stones in this way, and was in the habit of wearing a green diamond so coloured set in a ring.

It would not be fair to call Crookes a secretive man: nevertheless a certain sense of reserve was apparent to those who attempted to press him for information on scientific subjects. One well-known authority on electric discharge phenomena told me that he had given up asking Crookes questions—not because information was refused, but because he had a feeling that it was not willingly given. I remember at a later visit to his laboratory, Crookes generously showed me his unpublished investigation of uranium X, the first disintegration product of uranium, which afterwards became classical. He introduced it by saying that he had no objection to showing me a research which he had in hand. I was somewhat surprised by this turn of phrase, as it had never previously occurred to me that a scientific man could be unwilling to open his mind freely about matters like this, which had no commercial application. Crookes had done a good deal of commercial scientific work, being indeed dependent on it for an income, and possibly the attitude I have referred to may have been cultivated in connexion with it. He was also a good deal in

demand as an expert witness, and his name carried deserved weight, but, as a distinguished lawyer has told me, it was not much use to press him closely as to the logical implications of what he had said. He was an admirable experimentalist and a good man of business, but not a logician.

Looking over old letters, I find one from Crookes that contains some practical wisdom, and seems worthy of reproduction.

<div align="right">

7, KENSINGTON PARK GARDENS,

Nov. 14th, 1907.

</div>

Dear Mr Strutt,

Rutherford asked me to exhibit something at the Franco-British Exhibition, and I told him I really had nothing I could show. You ask me for an original spinthariscope. All my original ones were given away years ago, and now, when I want one I go to Beck's or Cossor's and buy one. But if I had anything to exhibit I doubt if I should be willing to do so. My experience is that an immense amount of trouble and some expense is involved. A case has to be provided, and fittings, labels, etc. got ready. Then the things to exhibit are so small that no one sees them. Scientific exhibits are of no interest to the public and men of science can see the same things far better elsewhere. Exhibitions are for large scale purposes. Had I a steam engine or a dynamo to show, I should send my men to put it up and take all the trouble, but here I have to do it all myself, and I really have no time to spare. I have some investigations half through, and I want to spend as much time in my laboratory as possible to get on with them.

I hope you will understand my position. You...with half a century in front of you can afford to be lavish of time, but I with my half century behind me, and naturally only a few years in front, must be parsimonious.

<div align="right">

Believe me,

Very truly yours,

WILLIAM CROOKES

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Mention has already been made of the joint work of Crookes and Dewar on water analysis. Crookes was a man of courteous and conciliatory manners: and I remember John Perry, one of our former Presidents, remarking that no one could possibly quarrel with him. Nevertheless Dewar accomplished the feat, and the partnership was, I suppose, dissolved. Both Crookes and Dewar were connected with the Royal Institution, the former as secretary and the latter as Fullerian Professor of Chemistry and director of the laboratory. There was therefore an *impasse*, and Crookes placed himself in the hands of the Duke of Northumberland as President of the Institution. The Duke took the view that as Crookes was to be President of the Royal Society, he could stand back without loss of dignity, and at the Duke's request he did so, though not very willingly. So far as I am aware, Crookes was never able completely to forget or forgive. But whatever his feelings may have been, he did not allow them to influence his actions. As President of the Royal Society he brought forward Dewar's name for the Society's greatest honour— the Copley Medal—and drew up a statement of Dewar's scientific merits in support. This was written with great care so as to do strict justice, without being in any way fulsome. The concluding sentence ran:

"The Nation owes Sir James Dewar a debt for his contributions to natural knowledge, and this debt the Royal Society will fitly recognize by conferring on him the Copley Medal."

Shortly after the declaration of war in 1914 Crookes brought forward a method of secret night signalling which he had worked out in collaboration with his assistant Mr Gardiner and which consisted in putting in or taking out a weak didymium glass from in front of a source of light. The general opacity was not enough to diminish the light appreciably, and it was not apparent to the naked eye that any change had been made, but anyone in the secret who watched with a direct vision prism, could readily observe the appearance and disappearance of the most conspicuous absorption band. I never heard that any use was made of this idea in practice, and it is perhaps chiefly interesting as showing the inventiveness of a man of eighty-two years of age, and his desire to do something for his country.

During the War Crookes served as a member of the Admiralty Board of Invention and Research. He was always treated with especial courtesy and consideration by Lord Fisher, the chairman, as was indeed fitting in view of his age and position. It cannot be said that he contributed much to the proceedings, and his activity was diminishing. But he went on to the end, his last paper appearing in his eighty-seventh year. He died while it was passing through the press.

I am indebted to Mr Gardiner for the photographs reproduced.

§ 5. SIR ARTHUR SCHUSTER, 1851–1934

I have always understood that Sir Arthur Schuster met with some initial opposition from his father in following his strong natural bent towards physical science. The father's own interests were all in the direction of finance and commerce: and he was almost as much perplexed and distressed as the traditional hen which has hatched out a duckling and finds it determined to take to the water. He came to Sir Henry Roscoe and expressed his annoyance and disappointment. But Roscoe insisted that he was making a mistake and that if he would only let his son have his own way, he would soon be proud of him: and so it proved.

Roscoe indeed had great and well-placed confidence in him. When on the death of Balfour Stewart the professorship of Physics at Manchester became vacant, Roscoe was earnest in promoting Schuster's candidature. The late Lord Rayleigh was consulted, and Roscoe was almost indignant with him for expressing any hesitation between Schuster and another very strong candidate. "Why won't you say straight out that Schuster is the best?" he said.

Schuster was one of the now fast diminishing band who derived personal inspiration from Clerk-Maxwell. He used to deplore the fact that the rising generation of physicists did not study Maxwell's great book on *Electricity and Magnetism* at first hand, but were content to derive their knowledge from secondary sources. He forgot perhaps the demands made on the student by the ever-widening range of scientific knowledge, and the necessity for learning established doctrine in whatever way may be the easiest.

I learnt from Schuster an aphorism by Maxwell which is perhaps not on record,

and this opportunity of preserving it may be taken. Some question being raised as to whether a particular scientific communication was worth publishing, Maxwell remarked: "Whether it is worth publishing depends on what ratio the ingenuity it displays bears to the whole ingenuity of the author." This consideration may very naturally guide authors themselves; but referees and publication committees usually attempt to apply a more objective standard of merit.

The following from Schuster to Rayleigh (August 1st, 1909) is worth preserving:

"There is much in these letters [from Maxwell to Rayleigh] that is interesting. If opportunity offers I think the one dated December 6th 1871 is well worth publishing.* I dont remember the 'moral' of the 2nd law [of thermodynamics] being put in the way it is done in this letter. The description of the consequence of reversal of motion of every particle in the universe I have seen ascribed to Kelvin. It is put in this letter as something original. As regards this matter it would no doubt appear very grotesque to an outsider if he could see people imagining that they are getting older when they are really getting younger, but should *we* notice anything strange about it? If anyone was to assert at the present moment that this reversal has taken place, and that all our recollections are really prophecies, and *vice versa* is there anything to disprove it? Here is the foundation of a new philosophy."

Schuster's scientific career has been set out in adequate detail by Professor Lees, and I shall not attempt to recapitulate what has been so well told by him. Schuster's style was essentially addressed to academic readers, and even in his more popular writings such as *Britain's Heritage of Science* he made, and desired to make, no appeal to the gallery. His enthusiasm was not obviously aroused by skilful technique or by ingenious mechanical devices. I have heard him speak as if he considered the development of wireless telegraphy a very much overestimated achievement, when compared with Hertz's fundamental experiments on electric waves. For him, science consisted mainly in the discovery of scientific facts and laws, not in applying them to the service of man. So well-balanced a mind could not fail to understand how great are the difficulties which inventors have to overcome: but these triumphs were not the things in which he personally delighted.

Schuster was near making some of the most important discoveries of his day. Two instances particularly come to mind. He used to tell how in early days he looked for the effect of a magnetic field on spectrum lines, subsequently discovered by Zeeman. Having arranged the experiment, with his eye to the spectroscope, he turned on the electromagnet and observed a large broadening of the lines. At first his hopes were high. But further investigation showed that what he had seen was to be explained by the magnet pulling apart the steel jaws of the slit!

Again, he was perhaps the first to realize the important information that could be gained by a quantitative measurement of the deflection of cathode rays in a magnetic field. He carried out measurements of this kind himself, and drew provisional conclusions, but these failed of success through his having over-estimated the effect of the resistance of the gas on the motion of the particles. I once happened

* It is now published in *Life of Lord Rayleigh*, p. 47.

to be with Schuster looking at a demonstration of X-ray apparatus. He remarked: "I owe a heavy grudge to Röntgen's discovery. It came out just at the time when I was working at the magnetic deflection of cathode rays. I laid my work aside and devoted all the resources of the laboratory to making and exhausting X-ray tubes, and taking photographs for the hospital at Manchester. The result was that other people got in first with the discovery of the electron."

It may be that if he had been gifted with a somewhat more vigorous physique, he would have been able to push these investigations to ultimate success in both cases. It is clear that he had the root of the matter.

As Secretary of the Royal Society, Schuster always took a broad point of view about the scientific work on which it was his duty to form a provisional judgment. With his great interest in the history of science, he had taken to heart the lesson that the orthodox view is not necessarily right. Thus on one occasion when the views of the late Professor A. W. Bickerton of Christchurch, New Zealand, on "impact" had come up, Schuster characteristically remarked: "We must give the heretics their chance." He was quite prepared to consider with an open mind the alleged phenomena generally called "spiritualistic", but modestly doubted his own capacity to detect trickery.

He thought that scientific bodies should not attempt to restrain men of established position from expressing any views they pleased. That, he considered, was outside their proper function. "If", he would say, "a man of standing chooses to make a fool of himself, that is his own affair, and I do not see that it is anyone else's business to stop him." When, late on in his life, a paper of his own was criticized by a referee, he took the view that at his age he had earned the right to say what he liked. He considered that the system of the French Academy of Sciences, which allots, as of right, a certain amount of space to each of its members, was worthy of imitation.

In the early phases of the war, the hysterical outbreaks of spy mania made difficult the position even of British subjects of long standing who had been born in Germany. Schuster was one of the sufferers, and the fact that he had installed at his house a wireless receiving set for getting the time from the Eiffel Tower station* gave a handle for the most grotesque misrepresentations. Attempts were even made to eject him from his position as Secretary of the Royal Society, but his friends stood by him, and, so far as can be judged, these attempts fell far short of success. To Schuster, however, all this came as a rude shock. Conscious of a life's work spent with complete loyalty in the service of the intellectual life of this country, it had not occurred to him that anyone could be so perverse as to take a different view: and when the ferment had subsided, he was not able to forget what had passed.

As a man of science, Schuster was singularly fair minded and unprejudiced. To the end of his life he remained free from the inflexibility of opinion which is so often the penalty of old age. For example, I ventured to ask him whether he still adhered to the opinion expressed in the preface to his *Optics* (1904), "Those who

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* This was, of course, before the days of broadcasting, and wireless receiving sets were not in general domestic use.

believe in the possibility of a mechanical conception of the universe, and are not willing to abandon the methods which, from the time of Galileo and Newton, have uniformly and exclusively led to success, must look with the gravest concern on a growing school of scientific thought which rests content with equations representing numerical relationships between different phenomena even though no precise meaning can be attached to the symbols used." "No," he replied, "I do not see how one can stand to that any longer."

Throughout a great part of his life his health was not very firm, though so far as I know he was not put out of action by long illnesses. He remarked once to me that he found that he could give an elementary lecture better when he had a slight headache, because that removed the temptation which he otherwise felt of allowing his mind to run on to other matters.

Even in hours of relaxation, the subjects on which he chiefly liked to dwell were science and the personalities and idiosyncrasies of scientific men. Politics hardly seemed to interest him at all, and he made no effort to join in when such subjects were under discussion. During his holidays he made charming water colour sketches which he would show with some legitimate pride to his friends. His interest in Art extended considerably beyond the range of that branch of it which he himself practised. But there I was not very competent to follow him. I remember his pleasure in a large piece of Della Robbia ware which was over the door of his dining-room at Manchester.

Schuster was a man of somewhat reserved exterior, and had a sensitiveness that would, perhaps, scarcely have been suspected by those who only knew him slightly. But beneath these characteristics lay a warm heart, and a capacity for sincere friendship. Towards the end of his life, when his health had finally given way, he said that to find himself remembered by his old friends was the only pleasure that remained to him.

Here I will bring this instalment of my recollections to a close. It would have been easy to cover a wider field: but I have preferred to exhaust a few topics rather than to touch lightly on many. If I have been able to communicate a part of the interest which I have found in recalling them, it will be an ample reward.

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ON THE DETERMINATION OF VISCOSITY BY THE OSCILLATION OF A VESSEL ENCLOSING A FLUID: PART I

By Professor E. N. da C. ANDRADE, F.R.S.,

AND

Y. S. CHIONG, M.Sc.

ABSTRACT. The method of measuring the viscosity of a liquid by observing the damping of torsional oscillations of a sphere containing the liquid was originally put forward by Helmholtz and Piotrowski. The form in which they carried out and calculated their experiments is ill adapted to give good results, and actually led to false conclusions. A disposition of apparatus and a method of calculation are described which can be made to yield accurate results. This has been checked by determining the viscosity of water over a range of temperatures.

§ 1. INTRODUCTION

THE usual method of determining fluid viscosity is based upon the flow through some form of tube. It is suitable for such substances as organic liquids at the limited range of temperatures in which determinations are ordinarily made, but for substances such as liquid metals, which react with air, and for temperatures either above or below the ordinary range, it possesses many disadvantages.

The apparatus which must be maintained at a constant temperature is relatively large, and any speck of oxide which may become lodged in the tube leads to grave errors. The usual procedure is to use gas pressure to reset the liquid for each flow, although K. Spells[1] has recently designed an apparatus in which the flow method can be used without contact between gas and liquid metal. The necessity of observing by eye the passage of the liquid past fixed marks is another feature of the method which leads to complication.*

The only other method which seems to have been used for liquid metals is that of the damping of an oscillating body, e.g. a disc immersed in the liquid, as applied by Fawsitt[2] and, more recently, by V. H. Stott[3] to the measurement of the viscosity of molten tin. This method, which is not absolute but only comparative, suffers from many disadvantages, of which the effect of the surface of the liquid on the rod connecting the suspension system to the disc may be particularly

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* Practical difficulties arise in the use of electrical contacts. See K. Spells[1].

mentioned. In general, it does not appear to be very well adapted for even comparative measurements.

A method which offers advantages is that originated by Helmholtz and Piotrowski[4], to which Dr Barr[5] refers as being "at least of historic interest". A sphere filled with the liquid is attached to a bifilar suspension and allowed to execute oscillations about a vertical axis. The logarithmic decrement is observed, and from this, and the constants of the apparatus, a value can be calculated for the viscosity. The original paper is certainly not such as to encourage others to use the method.* The calculations there given are very complicated and troublesome and the results very unsatisfactory. It was concluded by Helmholtz and Piotrowski that considerable slip took place between the liquid and the solid wall with which it was in contact, a result which has been quoted extensively, but is against the general experience of workers in this field[6]. The values found for the coefficients of viscosity differ, in one case very widely, from those now accepted—namely, 0·0157, 0·00251, 0·00360 for ether, alcohol and carbon disulphide at given temperatures in the neighbourhood of 20° C. as against the standard values, 0·0109, 0·00241, 0·00371 at the same temperatures. It is perhaps, therefore, not surprising that the method has not been extensively used. Ladenburg[7], however, repeated the measurements of Helmholtz and Piotrowski and was unable to find any evidence of slip; he himself obtained a value for water at 17·5° C. which differed by only 1 per cent from the standard value and a still better value at 19·2° C.

The method is so well adapted for measuring viscosities over a wide range of temperature on account of the ease with which relevant portions of the apparatus can be maintained at constant temperatures and with which the necessary readings can be taken, that we have devoted some attention to testing its validity and designing a form of apparatus suitable for the determination of the coefficient of viscosity to an accuracy of at least 0·5 per cent. It has been found possible to reduce the external damping—i.e. that which the oscillating sphere experiences owing to surrounding gas and the imperfect elasticity of the suspending fibres—so much that it can be practically neglected. This very much simplifies the calculation.

§2. CALCULATION OF THE COEFFICIENT OF VISCOSITY

The method of calculation adopted has been adapted from that used by Verschaffelt[8] for calculating the viscosity of a liquid gas from the damping of the oscillating of a sphere immersed in the liquid, and differs from that of Helmholtz in points that will be indicated.

The general equations of motion for a viscous fluid, summarized in vector notation as

$$\frac{\partial \mathbf{v}}{\partial t} + \mathbf{v} \operatorname{grad} \mathbf{v} = G - \frac{1}{\rho} \operatorname{grad} p - \nu \operatorname{curl} \operatorname{curl} \mathbf{v},$$

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* "On voit combien ces relations sont compliquées et quelle incertitude le calcul des expériences laissera planer sur la valeur de μ/f, qu'elles sont destinées à donner." Brillouin, *Viscosité des Liquides*, part 1, p. 102 (1907).

where **v** is the velocity, G the body forces, ρ the density, p the pressure and $\nu = \eta/\rho$ **v**, G, ρ, p, ν
the kinematic viscosity, reduce to the form

$$\left.\begin{aligned}\frac{\partial u}{\partial t} &= \nu \nabla^2 u \\[2mm] \frac{\partial v}{\partial t} &= \nu \nabla^2 v \\[2mm] \frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} &= 0\end{aligned}\right\} \qquad \dots\dots(1),$$

on the assumption that the liquid moves in concentric spherical shells about the
axis of rotation z, and that a given particle of liquid always remains in the same
plane normal to the axis, i.e. that $w = 0$, u, v and w being the components of velocity. u, v, w

We put $$u = -y\psi, \quad v = x\psi,$$ ψ

where ψ is a function of r, the distance from the centre of the sphere, and of t r
only, and we make the further assumption that

$$\psi (r, t) = \phi (r) e^{\alpha t},$$ ϕ

where $$\alpha = -\beta + i\gamma \qquad \dots\dots(2).$$ α

Here $\beta = \delta/T$, $\gamma = 2\pi/T$, where δ is the logarithmic decrement and T is the time β, γ, δ, T
period. We then find for ϕ the equation

$$\frac{d^2\phi}{dr^2} + \frac{4}{r}\frac{d\phi}{dr} = b^2\phi \qquad \dots\dots(3),$$

where $$b^2 = \alpha/\nu \qquad \dots\dots(4).$$ b

The solution of equation (3) is

$$\phi = \frac{1}{r^3}\{A (br - 1) e^{br} + B (br + 1) e^{-br}\} \qquad \dots\dots(5).$$

So far we have given a very brief summary of the method of Helmholtz, to
whom the reader is referred for details. Helmholtz proceeds from this point to
find an expression for $\psi = \phi (r) e^{\alpha t}$ in terms of β, fixed by the damping; γ, fixed by
the time period; σ and τ, which are functions of γ and β such that $\tau = \sqrt{(m/\nu)} \cos \epsilon$, m
$\sigma = \sqrt{(m/\nu)} \sin \epsilon$, where $m = \sqrt{(\beta^2 + \gamma^2)}$ and $\tan 2\epsilon = -\gamma/\beta$; and two complicated ϵ
functions of r, denoted by C and C_1. Hence he gets the viscous force P on the C, C_1, P
internal surface of the sphere in the form

$$P = Ke^{-\beta t} \cos (\sigma R + \gamma t + 2\epsilon + \kappa),$$

where $K = \frac{8}{3}\pi\rho\nu R^4 C$ and κ (called δ_1 by Helmholtz) is a function of R, the radius K, κ, R
of the sphere, introduced by a supposed slip between liquid and wall.

We then have the equation

$$I\frac{d^2\theta}{dt^2} = -f\theta - P - P' \qquad \dots\dots(6),$$

for the motion of the sphere, where P' is a term expressing the external damping P'
(mainly air friction), θ is the azimuthal angle and f is the restoring force per unit θ, f
angle of twist due to the suspension. This leads to

$$\eta = C/C_1 F,$$

F

when F is a complicated expression in m, R, I and f. This expression for η is quite intractable, and to simplify it Helmholtz takes $C_1/C = \sqrt{(m/v)}$ which, it should be noted, is only true if the dimensionless quantity $\sqrt{(m/v)} \, R$ is very large, say greater than 1000. As the value of this quantity was only 7·5 in Piotrowski's experiments with water, the anomalous results obtained are not surprising. To find the constants in P' a separate experiment has to be carried out on the damping of the oscillations of the empty sphere. Finally, after a troublesome calculation, a value of η is obtained.

We desire to simplify the calculation and to avoid making any assumptions not compatible with practical experimental conditions. Under the conditions of our experiments it will be shown that the external damping is responsible for a very small correction only, and can be neglected in the first instance. We start with equation (5), and, having to consider conditions at the inner surface of the sphere, where $r = R$, neglect e^{-bR} in comparison with e^{bR}. To a first approximation the real part of b is, as will be shown later, $\sqrt{(\pi/Tv)}$—see equation (14)—which is not less than 3·6 in our experiments, while R for our smaller sphere is 2, so that the neglect is well justified. If the value of ϕ at $r = R$ is Φ, then

$$A = \frac{\Phi R^3}{bR - 1} \, e^{-bR},$$

$$\phi = \frac{\Phi R^3}{r^3} \frac{br - 1}{bR - 1} \, e^{-b\,(R-r)},$$

and
$$\left(\frac{d\phi}{dr}\right)_{r=R} = \frac{\Phi}{R} \frac{b^2R^2 - 3bR + 3}{bR - 1}.$$

The angular velocity in any spherical shell is

$$\psi = \phi\,(r)\,e^{\alpha t},$$

so that
$$\frac{d\psi}{dr} = \frac{d\phi}{dr}\,e^{\alpha t},$$

and
$$(\psi)_R = \frac{d\theta}{dt} \qquad\qquad \text{......(7).}$$

X

Supposing, in accordance with all modern experiment, that there is no slip between liquid and wall, the viscous torque on an elementary zone, of latitude χ, measured from the equator, is

$$dP = 2\pi R^4 \eta \, \cos^3 \chi \cdot d\chi \left(\frac{d\phi}{dr}\right)_R e^{\alpha t},$$

or
$$P = 4\pi R^4 \eta \left(\frac{d\phi}{dr}\right)_R e^{\alpha t} \int_0^{\pi/2} \cos^3 \chi \cdot d\chi$$

$$= \frac{8}{3}\,\pi\eta\,\frac{R^4}{\Phi}\left(\frac{d\phi}{dr}\right)_R \frac{d\theta}{dt}$$

$$= \frac{8}{3}\,\pi\eta R^3 \left(bR - 2 + \frac{1}{bR - 1}\right)\frac{d\theta}{dt} \qquad\qquad \text{......(8)}$$

L

$$= L\,\frac{d\theta}{dt} \quad \text{say} \qquad\qquad \text{......(8\,}a\text{).}$$

If we put $L = M + iN$ and $b = g + ih$, then $\qquad$ M, N, g, h

$$M = \frac{8}{3}\pi\eta R^3 \left\{ gR - 2 + \frac{gR - 1}{(gR - 1)^2 + h^2 R^2} \right\}$$

$$N = \frac{8}{3}\pi\eta R^4 h \left\{ 1 - \frac{1}{(gR - 1)^2 + h^2 R^2} \right\} \qquad \dots\dots(9).$$

If there is no external damping, equation (6) becomes

$$I\frac{d^2\theta}{dt^2} = -f\theta - P \qquad \dots\dots(10).$$

From equation (7) $\qquad \dot\theta = \frac{1}{\alpha}\Phi e^{\alpha t},$

so that $\qquad I\alpha^2 + L\alpha + f = 0.$

Substituting the value of α from equation (2) we have

$$\begin{aligned} I(\beta^2 - \gamma^2) - \beta M - \gamma N + f = 0 \\ -2I\beta\gamma - \beta N + \gamma M = 0 \end{aligned} \qquad \dots\dots(11),$$

which gives

$$M = \beta \left\{ \frac{f}{\beta^2 + \gamma^2} + I \right\},$$

$$N = \gamma \left\{ \frac{f}{\beta^2 + \gamma^2} - I \right\}.$$

Remembering that

$$\beta = \delta/T, \quad \gamma = 2\pi/T, \quad f = \frac{4\pi^2 I}{T_0^2},$$

where T and T_0 are the periods of oscillation of the full and empty sphere re- $\qquad$ T, T_0
spectively

$$M = \frac{I\delta}{T}\left(\frac{T^2}{T_0^2}\frac{1}{\Delta^2 + 1} + 1\right)$$

$$N = \frac{2\pi I}{T}\left(\frac{T^2}{T_0^2}\frac{1}{\Delta^2 + 1} - 1\right) \qquad \dots\dots(12),$$

where $\qquad \Delta = \beta/\gamma = \delta/2\pi,$ $\qquad$ Δ

or to a good approximation, if Δ is small*

$$M = \frac{2\pi I\Delta}{T}\left(\frac{T^2}{T_0^2} + 1\right)$$

$$N = \frac{2\pi I}{T}\left(\frac{T^2}{T_0^2} - 1\right) \qquad \dots\dots(13).$$

Equations (12) and (13) contain only measured quantities, so that M and N
are known.

Now $\qquad b = g + ih$

$\qquad\qquad = \sqrt{(\gamma/\nu)}\,(1 + i)\,(1 + i\beta/\gamma)^{\frac{1}{2}},$

since $\qquad b^2 = \alpha/\nu = 1/\nu\,(-\beta + i\gamma),$

---

* In a typical experiment, with water at $12 \cdot 7°$ C., δ was $0 \cdot 04789$, or $\Delta^2 = 5 \cdot 8 \times 10^{-5}$.

which gives

$$g=\sqrt{\frac{\pi}{T\nu}}\left(1-\tfrac{1}{2}\Delta+\tfrac{1}{8}\Delta^2\right) \Bigg\}$$

$$h=\sqrt{\frac{\pi}{T\nu}}\left(1+\tfrac{1}{2}\Delta+\tfrac{1}{8}\Delta^2\right) \Bigg\} \qquad \ldots\ldots(14),$$

neglecting Δ^3 and higher powers. To a first approximation

$$g=h=\sqrt{\frac{\pi}{T\nu}}.$$

Referring to equations (9) which give η in terms of M and N (which have been found), g, h, and known quantities, we see that we have sufficient equations to find η, since g and h involve only η, T and ρ. However, since g and h contain $1/\sqrt{\eta}$, the exact evaluation of η gives a little trouble. This we can avoid by a method of successive approximations.

The quantity $(gR-1)^2+h^2R^2$, which occurs in equations (9), is large, being, e.g. about 80 for water near $0°$ C. with our smaller sphere, and greater in other cases. Hence, to a first approximation,

$$N=\frac{8}{3}\pi\eta R^4\sqrt{\frac{\pi}{T\nu}},$$

or

$$\eta=\frac{9N^2T}{64\pi^3R^8\rho}$$

$$=\frac{9}{16\pi}\frac{I^2(T+T_0)^2(T-T_0)^2}{TT_0{}^4R^8\rho} \qquad \ldots\ldots(15).$$

In the value for M given by equations (9) let

q

$$q=\frac{gR-1}{(gR-1)^2+h^2R^2} \qquad \ldots\ldots(16),$$

which is of the order 0.04. The value of gR is of the order 15, so that q is only a small correction, and the value of η given by equation (15) can be used to find its value. We then have, from the equation for M in equations (9),

$$(2-q)\eta-\sqrt{\frac{\pi\rho}{T}}\,aR\sqrt{\eta}+\frac{3}{8}\frac{M}{\pi R^3}=0,$$

or

$$q'\eta-\sqrt{\frac{\pi\rho}{T}}\,aR\sqrt{\eta}+\frac{3}{4}\frac{I\Delta}{R^3T}\left(\frac{T^2}{T_0{}^2}+1\right)=0,$$

q', a where

$$q'=2-q, \qquad a=1-\tfrac{1}{2}\Delta+\tfrac{1}{8}\Delta^2.$$

Hence

$$\sqrt{\eta}=\frac{aR}{2q'}\sqrt{\frac{\pi\rho}{T}}\{1-(1-\mu)^{\frac{1}{2}}\},$$

μ where

$$\mu=\frac{3q'I\delta}{2\pi^2a^2R^5\rho}\left(\frac{T^2}{T_0{}^2}+1\right),$$

or

$$\eta=\frac{a^2R^2}{4q'^2}\frac{\pi\rho}{T}\{1-(1-\mu)^{\frac{1}{2}}\}^2 \qquad \ldots\ldots(17).$$

While equation (15) should apparently give η within a few per cent, as far as the approximations made are concerned, actually $(T-T_0)^2$ is the main source of error, since T and T_0 are nearly equal. Owing to this, the first approximation for η may be in error by 30 per cent or more. The effect of this will be to render this value given by equation (17) in error by 1 or 2 per cent. It is then, however, only necessary to use this new value of η to find q', and to work through equation (17) again with the new value of q'. This stage of approximation gives η correct to well within the limits dictated by the various experimental errors. The third approximation is very quickly carried out, as the values of all the quantities involved in equation (17) except q' have been already calculated in the second approximation. In an actual series of experiments, and in finding the temperature coefficient of viscosity, it is generally possible to guess the first value of η to within a few per cent, instead of using equation (15); if so, the second approximation gives the required accuracy.

As an example we may take an experiment carried out on water at 13·4° C.

$$\delta = 0\cdot11236, \qquad I = 927\cdot14 \text{ g.cm.}^2,$$
$$\rho = 0\cdot99935 \text{ g./cm.}^3, \qquad T = 15\cdot113 \text{ sec.},$$
$$T_0 = 14\cdot719 \text{ sec.}, \qquad R = 2\cdot544 \text{ cm.}$$

With those values equation (15) gives

$$\eta = 0\cdot0171, \qquad g = 3\cdot487,$$

and we find

$$q' = 1\cdot943, \qquad \mu = 0\cdot6044,$$

from which equation (17) gives $\eta = 0\cdot01176$ as a second approximation, which differs so much from the first approximation that a third approximation is necessary.

Putting $\eta = 0\cdot01176$, we get

$$g = 4\cdot2020, \qquad q' = 1\cdot9534, \qquad \mu = 0\cdot60746,$$

from which equation (17) gives $\eta = 0\cdot01207$.

To show how unnecessary it is to proceed further it may be mentioned that the effect of a fourth approximation is to give $\eta = 0\cdot012068$, instead of $0\cdot012069$ given by the third approximation.

§ 3. APPARATUS

The suspension was a bifilar one, which has obvious advantages. The first difficulty is that of obtaining a pure rotation, free from any swing. After electro-magnetic methods of starting the oscillation had been tried, a mechanical arrangement was finally adopted.

The suspension head, shown in figure 1, consists of a truncated cone A fitting into a cylindrical block BB, which is fixed to a levelling table FF, the levelling screws being of the type used in the standard dumpy level. The cone was turned in the lathe so that the flat top was accurately normal to its axis, and at the same time a small coaxial conical hole was turned in the small end of the cone. When the suspension wires are fixed at opposite ends of a diameter of this hole, by means of
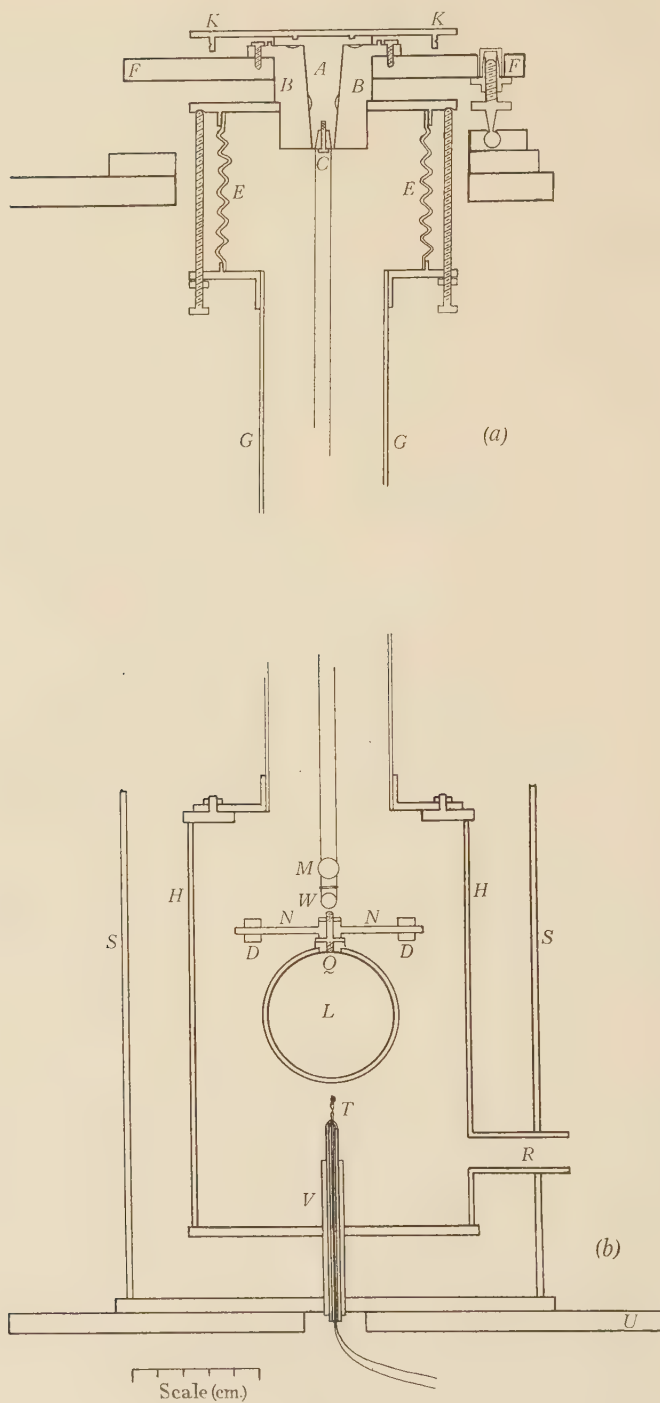
Figure 1.

a plug C, which fits tightly into it, they must be equidistant from the axis of the suspension head, and when the top of the head is accurately levelled the axis must be truly vertical. To aid in the levelling a table KK is provided.

A rotation of the head will impart a true rotation to the sphere if it is hanging with its centre of mass on the truly vertical axis of the conical head. To ensure this the bifilar suspension is made of a single wire, the loop of which carries a small wheel, W, provided with a fine circumferential groove, figure 2. The frame TT (seen from front and side in figure 2 a and b) which also carries the sphere, is provided with a horizontal bar just above the wheel, whose width is slightly less than
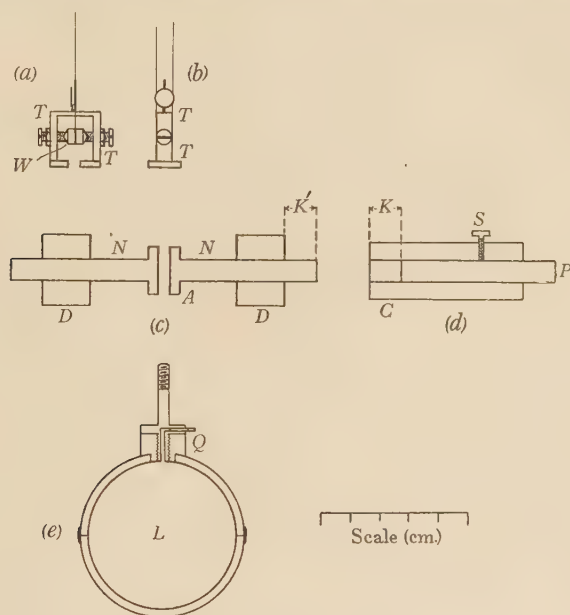
Figure 2.

the diameter of the groove. The two wires, therefore, just clear the sides of the bar, which are covered with a thin layer of sealing wax, except near the points where the wire passes. After the suspension is in position, and the centre of the mass of the whole suspended body has settled in the axis of the head, the wax is melted with a special electric heater, and fixes the suspension wires rigidly to the bar, without having to bear any weight. In this way any uncertainties of motion, which would occur if the sphere were suspended by the wheel, are eliminated.

The frame TT is fastened by nut and screw to the block Q, which is attached to the sphere L and also carries the inertia bar NN. The hole in Q, used during the filling, is closed by a plug. The whole suspension and sphere is in a vacuum-tight enclosure, as shown in figure 1. This consists of a glass tube GG, a metal box HH, surrounded by a water jacket and suitable windows, and a tombac bellows EE, connecting the brass plate holding the glass tube to the brass plate soldered to BB. This bellows was found convenient in avoiding any strain on the suspension head

or on the glass tube. Long metal screws, shown in the diagram, were used to prevent the bellows collapsing when the system was evacuated. The evacuation was carried out with a mercury vapour pump, suitably backed, and the vacuum produced was estimated at about 10^{-5} mm. of mercury, or less. The temperature was taken by the thermocouple T.

The arrangement shown in figures 1 and 2 is designed for the accurately ground sphere (see § 5), and was adopted for a limited range of temperature, in the neighbourhood of room temperature, only. Spheres blown on the end of tubes were also used, especially for higher temperatures. Such a sphere is shown in figure 3,
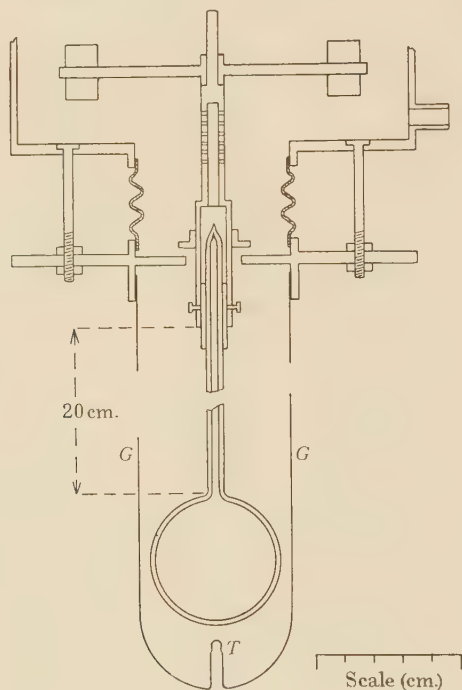
Figure 3.

with the inertia bar attached. The suspension arrangement was the same as that shown in figure 1. The sphere was surrounded by a glass tube GG, the depression T being designed to receive a thermocouple. The heating was done by means of an electric furnace surrounding the whole of the tube GG except the uppermost few inches, which were kept cool by a water jacket.

The amplitude was recorded photographically. A celluloid scale was mounted so as to form the arc of a circle of about 1 metre radius, and fixed so that the moving mirror lay at its centre, the path of the beam from the mirror to the scale being included in a light-tight box. A strip of photographic paper was then fastened to the far (graduated) side of the celluloid. With light of a suitable strength the extremities of the swing left a clear trace on the paper, on which the scale was also recorded, so that deformation of the paper during printing and drying did not

affect the accuracy of the results. From the photographic record the logarithmic decrement was calculated. Successive experiments at the same temperature gave differences in the decrement not exceeding 1 part in 2000.

The correction for external damping was made by taking the logarithmic decrement with the empty sphere. A swing of amplitude 20 cm. diminished by not more than 1 cm. in an hour. The correction varied from 0·5 to 0·8 per cent with the different spheres.

The expansion of the glass with temperature is so small over the range of 60° C. (coefficient of linear expansion of pyrex glass is 3×10^{-6} per deg. C.) that the correction for change of R with temperature is practically negligible, the extreme variation causing a change of about 0·1 per cent in the calculated value of η.

§ 4. THE DETERMINATION OF THE MOMENT OF INERTIA OF THE SUSPENDED SYSTEM

The moment of inertia of the suspended system must be accurately known, as the calculation of η involves the square of this quantity. There are two main difficulties: firstly to get standard inertia bars, of equal weight but different values of moment of inertia, which must be sufficiently true for the moments of inertia to be calculated with the required accuracy from their geometrical measurements; and, secondly, to avoid any change in the suspension wires, due to tension or other causes, when the inertia bars are interchanged. The second difficulty can be avoided by careful manipulation: to meet the first the following disposition was adopted.

The standard inertia pieces consist of a central rod and two thick discs, DD, figure 2c. To ensure uniformity in density, and true geometrical form, the rod and the weights are cut from a single large block of brass and carefully turned in the lathe. In the middle of the rod is left a block A, with a central hole through which the rod and the weights can be attached to the oscillating system. There is close sliding fit between the weights and the rod.

Measurement in an accurate gauge, kindly put at our disposal by the Engineering Department of University College, showed that the greatest variation in diameter in one disc was 1 part in 4000, while in the other it did not exceed 1 part in 20,000; the mean diameter of the two discs differed by 1 part in 5000. The thickness showed a like uniformity and the diameter of the central rod had as extreme values 0·4770 and 0·4775 cm. To adjust the distances of the weights from the centre of the rod a special gauge was made, shown at CP. This consisted of a cylinder C with an adjustable piston P, fixed in position by the screw S, figure 2d. The depth K of the cylindrical cavity having been carefully measured with a depth gauge reading to 0·01 mm., the gauge is then pushed over the rod, displacing the disc D. The distance K' is then accurately equal to K, the nature of the contacts made by the depth gauge being very similar to those made in the adjustment. It may be mentioned that a groove is cut along the rod of the gauge to give an outlet for the trapped air. Three different moments were used, and three different gauges were made and kept fixed, one for each adjustment.

I_1
I_n

The moment of inertia of the system is made up of a fixed part I_1 due to the sphere and inertia rod, and a variable part I_n, due to the movable discs in position n, which can be calculated.

We have
$$T_n = 2\pi \sqrt{\frac{I_1 + I_n}{f}},$$

T_n

where T_n is the time period corresponding to I_n.

The restoring couple f is independent of the moment of inertia, the load being the same in all cases. Any two values of n enable I_1, and hence the total moment of inertia actually used in the experiment to be found. Three different values were actually used giving two independent values of I_1. In this way the moment of inertia was found with an error of not more than 1 part in 2000, which means an error in η of not more than 1 part in 1000.

§ 5. THE SPHERES

Five spheres were used in all. The first was a glass sphere 5·088 cm. in diameter, made in two halves, the glass being about 3 mm. thick. This sphere had been prepared for another purpose, and was stated to be true to within 0·02 mm., or 1 part in 2500. A hole in the one hemisphere received the brass block carrying the inertia bar and mirror, which was cemented in, and the two halves were fastened together by means of a brass ring and cement.

The other spheres were selected from a number of spheres of pyrex glass blown at the end of a glass tube. The diameters in various directions were measured by immersing the sphere in a mixture of benzene and methyl alcohol, of the same refractive index as the glass, and observing with a reading microscope directly on the inner surface of the glass. A 1-per-cent difference in the refractive indices gives rise to a 1-per-cent error in the measurement, so that the method is not satisfactory from the point of view of absolute accuracy unless the mixture is very carefully prepared. Our method was to use an approximately correct mixture in a preliminary test, to estimate the departures from sphericity, and later to make up the mixture very carefully, and measure the diameter in the neighbourhood of the equator, for reasons to be given shortly. The spheres were in general slightly flattened at the poles, but the greatest difference of diameters with those used was 0·04 cm. on a diameter of 4 cm. or so, or about 1 per cent.

The absolute value of R was estimated by weighing the sphere full of water, which gives, of course, a mean value of R^3. It should be noted that 88 per cent of the volume of the sphere is contributed by a zone bounded by planes passing through the latitudes of $\pm 45°$.

As the viscous torque on the sphere varies as $\cos^3 \chi$, the slight flattening matters very little, since 88 per cent of the damping takes place over the zone for which $\chi = \pm 45°$. Table 1 gives the calculated percentage of the damping due to different zones extending to either side of the equator up to the angle specified. It will be seen that the 30° in the neighbourhood of the pole ($\chi = 60°$ to $\chi = 90°$) contributes only 2·5 per cent of the damping, the 20° only 1 per cent and the 10° only 0·05 per

cent, so that the exact shape of the sphere when the tube joins it is immaterial. The average value of R from $\chi = 0°$ to $\chi = 45°$ was found to agree, in all cases, to within 1 part in 4000, with the value obtained by weighing, which, as we have seen, lays stress on the zone in question, and was therefore adopted.

Table 1

Latitude	10°	20°	30°	40°	50°	60°	70°	80°	90°
Percentage of total damping	25·8	49·3	68·7	83·1	92·4	97·3	99·4	100	100

§6. RESULTS

With the object of testing the method the viscosity of water was measured over a temperature range of from 2·5 to 65·2°, the five different spheres being used and two suspensions. The results are shown in table 2 and figure 4, the smoothed

Table 2. Viscosity of water

Sphere	Temperature	η observed	η (I.C.T.)	Percentage difference	Probable error (%)
A	2·5	0·01657	0·01647	+0·6	0·3
	5·6	0·01479	0·01491	−0·8	
	13·2	0·01208	0·01200	+0·7	
	15·4	0·01130	0·01133	−0·3	
	22·4	0·00951	0·00952	−0·1	
B	12·7	0·01212	0·01216	−0·3	0·3
	25·0	0·00895	0·00895	0·0	
	31·3	0·00772	0·00778	−0·8	
	51·7	0·00529	0·00534	−1·0	
C	17·5	0·01061	0·01073	−1·1	(0·6)
	65·2	0·00439	0·00436	+0·7	
D	17·5	0·01076	0·01073	+0·3	(0·4)
	40·0	0·00660	0·00654	+0·9	
E	19·8	0·01015	0·01014	+0·1	(0·1)
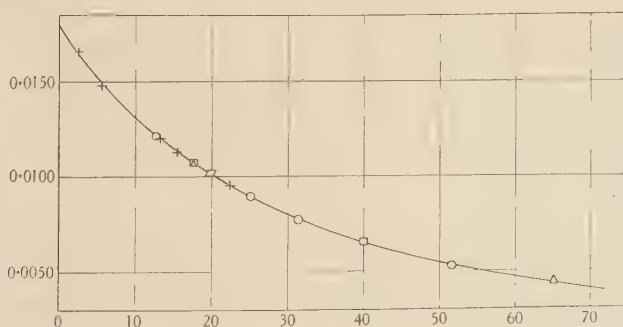
Figure 4. Sphere A, $+$; sphere B, $\bigcirc$; sphere C, $\triangle$; sphere D, $\square$; sphere E, $\diamondsuit$.

figures from the International Critical Tables (I.C.T.) being taken as standard. The probable errors for the determinations made with each sphere are given. The

worst value is for C, where there was a little uncertainty in the temperature. At the lower temperature a change of 1 per cent in the viscosity corresponds to an error of about 0·3° C., at the higher temperatures to an error of 0·8° C. No special precautions were taken to keep the temperature constant, and probably part of the error in the determination was due to fluctuations of temperature. It seems a fair conclusion for these experiments that, even with a blown glass sphere, not specially worked, the absolute value of the viscosity can be determined to within 0·5 per cent.

We can say, then, that the sphere method is probably capable of as great accuracy as any of the standard methods, and possesses the many advantages pointed out in the introduction. It is now being applied successfully to measure the viscosity of the molten alkali metals, and results will shortly be published.

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DISCUSSION

Dr ALLAN FERGUSON. A relatively simple treatment of the problem of the oscillating sphere is to be found in Lamb's *Hydrodynamics*—a treatment which, so far as I remember, does not discuss the finer points of technique in the admirable manner adopted in these papers. It is subject, of course, to the βa condition, and the experimental conditions could be arranged to give results of the one or two-per-cent order of accuracy for fluids having viscosities of the order of that of water. I have found it convenient to determine moments of inertia by loading the sphere with an anchor ring.

ON THE DETERMINATION OF VISCOSITY BY THE OSCILLATION OF A VESSEL ENCLOSING A FLUID: PART 2

By Professor E. N. da C. ANDRADE, F.R.S.,

AND

LEONARD ROTHERHAM, M.Sc.

ABSTRACT. A new method of carrying out the oscillating sphere method is described. The sphere containing the liquid carries a small permanent magnet, at the centre of a Helmholtz coil system. A condenser, discharged through the coils once in every complete swing, supplies the energy dissipated by the viscous forces, and so maintains a constant amplitude. Measurement of this amplitude, and of the potential of the condenser, enables comparative measurements of viscosity to be accurately made. The method has been checked by measuring the viscosity of hexane in terms of that of water. Absolute measurements are possible by the method, but have not been carried out.

§ 1. INTRODUCTION

IN the method of measuring viscosity originated by Helmholtz and Piotrowski, to which part 1 of the paper is devoted, the energy of the oscillation is gradually dissipated by the non-conservative viscous forces, and the viscosity is determined by measuring the damping. It is possible, however, by supplying energy to the system at every oscillation to maintain a fixed amplitude, the amount of energy supplied to the system being then equal to that dissipated. The viscosity can then be calculated from the amplitude and the energy communicated.

The principle of the method to which this paper is devoted is as follows. To the sphere, suspended as before, is rigidly attached a short magnet of high coercivity. A pair of Helmholtz coils is arranged so that the magnet lies at the centre, with its axis normal to the axis of the coils when the sphere is at rest. By means of a beam of light reflected from a mirror attached to the sphere and a light sensitive relay a condenser of known capacity, at a known potential, is discharged through the coils when the oscillating sphere passes through its equilibrium position. An impulse proportional to the quantity of electricity passing through the coils is thus given to the suspended system within a time which is very short compared to the period, just as in a ballistic galvanometer through which a condenser is discharged. The amplitude of the swing will increase (or decrease) until the energy dissipated by the motion of the fluid is just equal to that supplied. As it is difficult

to set up mechanically an oscillation without pendulum swing, except by a very carefully constructed system (see part 1) it is best to start with the sphere at rest, and let the condenser discharge work up the oscillation to its final amplitude.

§ 2. CALCULATION OF THE VISCOSITY

From equations (8) and (8 a) of part 1 the moment of the viscous forces on the sphere is

$$P = \frac{8}{3}\pi\eta R^3 \left(bR - 2 + \frac{1}{bR-1}\right)\frac{d\theta}{dt} = L\frac{d\theta}{dt}.$$

J The change of angular momentum J in time dt is therefore

$$dJ = L.d\theta,$$

or the change of momentum in one complete oscillation is

$$J = 2L(\theta_1 + \theta_2) = 2L\theta_A,$$

$\theta_1, \theta_2, \theta_A$ where θ_1 and θ_2 are the extreme angular displacements, and θ_A is total angular displacement.

Taking the real part of L

$$J = 2M\theta_A$$

$$= \frac{16}{3}\pi\eta R^3 \left\{gR - 2 + \frac{gR-1}{(gR-1)^2 + h^2 R^2}\right\}\theta_A \qquad \dots\dots(\text{II, 1})$$

Now in $$\left. \begin{aligned} g &= \sqrt{\frac{\pi}{T\nu}}.(1 - \tfrac{1}{2}\Delta + \tfrac{1}{8}\Delta^2) \\ h &= \sqrt{\frac{\pi}{T\nu}}.(1 + \tfrac{1}{2}\Delta + \tfrac{1}{8}\Delta^2) \end{aligned} \right\} \qquad \dots\dots(\text{I, 14}),$$

the value of Δ is small, being round about 0·02 or less in the present experiments. The term $\tfrac{1}{2}\Delta$ therefore represents a correction of about 1 per cent, while the term in Δ^2 is negligible. Hence a very rough value of the logarithmic decrement δ is sufficient: even an error of 10 per cent in δ makes an error of only about 0·1 per cent in J.

M The momentum supplied by the condenser discharge is MGQ where M is the
G moment of the magnet, G is the field produced at the centre of the coils by unit
Q, C current and Q is the quantity of electricity discharged through the coils. If C is
V, x the capacity of the condensers in farads, V the potential in volts, x the radius of
n the Helmholtz coils, of n turns, then

$$GQ = \frac{0.899n}{x}CV.$$

If the moment of the magnet is measured, we can find a value for J in equation (II, 1), and hence an absolute value of the viscosity. In the above the damping due to residual gas in the evacuated space and imperfect elasticity of the suspension is neglected. It is, in fact, negligible in our experiments. Allowance can easily be made for it, if necessary, by finding, with the aid of a condenser of small capacity, the energy necessary to maintain the empty sphere in oscillation at a fixed amplitude.

It is, however, easier to calibrate the apparatus by using a liquid of known viscosity, preferably water. The method of proceeding is then as follows.

From equation (II, 1)

$$\eta \left\{ gR - 2 + \frac{gR - 1}{(gR - 1)^2 + h^2 R^2} \right\} s = \frac{3MCL}{16\pi R^3} V$$

$$= KV \qquad \qquad ......(\text{II}, 2),$$

where L is the distance of the circular scale from the mirror at its centre, s is the total length of the arc of swing, and K is constant for a given sphere and a given capacity. The values of s, T and Δ having been found with a liquid of known η and ρ, and with a determined value of V, the constant K is known.*

Using the same sphere and capacity, but, if convenient, a different V, we find s, T and δ.

Since $g = \sqrt{(\pi/T\nu)}$ approximately, while

$$\frac{gR - 1}{(gR - 1)^2 + h^2 R^2} = \frac{1}{2gR},$$

approximately, and is small (about 0·05 in these experiments) and can be neglected in comparison with $gR - 2$ (about 10), we can write

$$\eta \left(\sqrt{\frac{\pi \rho}{T} \cdot \frac{1}{\sqrt{\eta}}} - 2 \right) s = KV \qquad \text{q.p.} \qquad ......(\text{II}, 3),$$

which, using our value of K, gives us a quadratic to find a first approximation for η. With this value of η we find an approximate value for $gR - 1/\{(gR - 1)^2 + h^2 R^2\}$ and, taking account this time of the $\frac{1}{2}\Delta$ term in g, solve the quadratic given by equation (II, 3) for η. If necessary a third approximation can be made.

As an example of the rapidity of convergence, using an experiment on water at 0°, with standard value $\eta = 0·01797$, to find K, equation (II, 3) gives, for an experiment on water at 25·0° C.,

$$\eta = 0·00907,$$

as a first approximation, while the second approximation is

$$\eta = 0·00902 \quad (0·009019).$$

A third approximation is unnecessary: it gives 0·009016. The standard value is 0·00895.

§ 3. EXPERIMENTAL ARRANGEMENT

The sphere used was the accurately worked one, made in two halves cemented together, described in part 1. It was suspended by a bifilar arrangement from a plate BB, figure 1, supported by a massive beam, the lower end of the suspension being attached to a brass piece carrying the magnet P and two mirrors inclined at an angle to one another, one to reflect the beam operating the selenium relay, and the other to register the amplitude of the oscillations in the ordinary way. This

* The capacity C can, of course, be varied if desired, $K'CV$ then taking the place of KV, and K' being determined.

brass piece was rigidly fastened to a glass rod, some 20 cm. long, carrying the sphere at its lower end. The method of attachment of the suspension ensured that the centre of mass of the sphere lay in the line midway between the two vertical and parallel suspension wires.*

The whole suspended system was surrounded by a vacuum-tight enclosure attached to the plate BB. It consisted of a tombac bellows D, to allow flexibility during adjustment; a glass tube HH, about 82 cm. long; and a brass tube system,
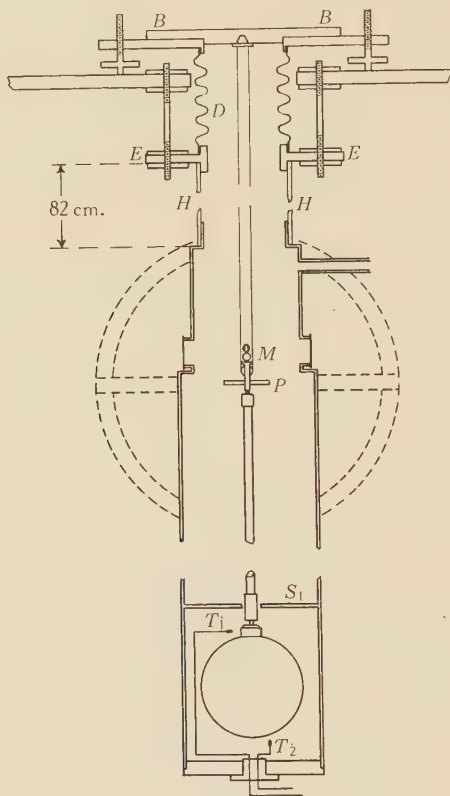
Figure 1.

provided with suitable windows to which the Helmholtz coils were rigidly attached. When the necessary adjustments had been made the plate EE was fixed by the screws shown, to avoid collapse of the bellows on evacuation.

To discharge the condenser automatically through the coils at the moment when the sphere passed through its equilibrium position a selenium cell with relay was employed. The usual simple device, by which the passage of the light beam across the cell operates a relay, is not suitable in the present instance, since there are two passages per complete oscillation which would produce identical results, and so lead to an impulsive force of one sign for the direct passage, and of the opposite sign for the reverse passage, which cancel out.

* See part 1, p. 255 and figure 1.

An optical arrangement was therefore used by which the cell is shielded from the light during one half of the complete oscillation, and illuminated during the other half, a suitable bridge arrangement ensuring that the condenser was charged when the cell was in darkness, and discharged when it was first illuminated. Light from a slit S is concentrated by the lens L on the plane mirror at M, and comes to a focus at F in the plane of the screen CD, being just at D when the mirror is in the equilibrium position (figure 2). A large plane-convex condensing lens BB is arranged with its plane face against the screen CD, and the selenium cell G is placed where BB forms an image of the surface of M. The large condensing lens does not, of course, give a sharp image but is good enough to ensure that sufficient light falls upon the cell for all positions that F actually takes up on the clear half of the lens.
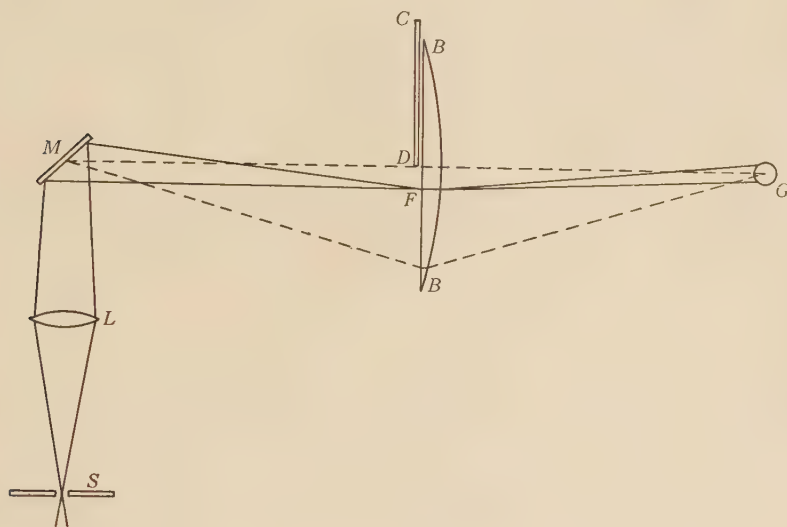
Figure 2.

In this way, instead of having two identical dark-light-dark passages per complete oscillation, we have one dark-light passage and one light-dark passage, which produce the required results. A separate mirror is used to record the amplitude.

Bright tin radiation shields, of which the lower one, S_1, is shown, helped to keep the temperature uniform in the enclosure. The temperature was recorded by two copper-constantan thermocouples T_1, T_2, one just above the other just below the sphere. They gave the same reading within $0 \cdot 1°$ C. or less. The brass enclosure surrounding the sphere was jacketed with water kept at a thermostatically controlled constant temperature, or with water containing melting ice.

The pressure inside the enclosure was maintained at about $0 \cdot 001$ mm. of mercury or lower by means of a mercury vapour pump with the usual backing. The air damping appeared to be negligible. However, as was pointed out in § 2, the exact value of the (small) external damping is not necessary, as the whole damping appears only as a small correction in the final result.

§ 4. EXPERIMENTAL TEST OF METHOD

The method has not been used to find an absolute value of the viscosity, which demands an accurate measurement of the magnetic moment and a careful consideration of the field due to the coils, but only for comparative measurements. The apparatus can be easily calibrated with a suitable known liquid. In the present case we have used water, and carried out the following tests: (1) We have found two values of K, as described in § 2, by using water at two different temperatures. The two values agree well. (2) Using the value of K so derived we have found the viscosity of hexane, which is much less than that of water, at two different temperatures, and compared them with the standard values.

The two temperatures selected for water were 0·0° C. and 25·0° C. Taking the values of η as being respectively 0·01797 and 0·00895, and substituting the appropriate values in equation (II, 2) we find $K = 0·04661$ and 0·04669 respectively. As an example the actual figures for water at 0·0° C. were $\delta = 0·150$, $s = 38·084$ cm. (mean value for 30 swings, the extreme values being 38·06 and 38·16), $T = 9·014$ sec., $V = 133·8$ V., $R = 2·544$ cm.

Using the mean value of $K = 0·04665$, experiments on hexane yielded the following values, η being the value found in the experiments, and η_s the standard value, extrapolated from Thorpe and Rodger's value, with m taken as 1·1 in the Hagenbach-Couette correction.* The agreement is disconcertingly good.

Temperature (°C.)	η	η_s
0	0·003956	0·003965
21·9	0·003135	0·003137

§ 5. ADVANTAGES OF THE METHOD

The method possesses, of course, the advantages of any method in which the liquid is enclosed in a sealed vessel throughout the experiment, to which allusion has been made in part 1. As against the method there described it has certain additional merits. (1) It is in some respects easier to set up the apparatus in the first case, as the carefully worked suspension head is eliminated. (2) The actual readings to be made are much simpler. Instead of the photographic registration of a long series of swings, which is necessary if the logarithmic decrement is to be found accurately, there is only the (constant) amplitude of swing to be noted. (3) The calculation is shorter. (4) The correction for the inherent damping of the system due to the suspension wires and the residual air is very simply made if it is necessary to take it into account.

As regards accuracy, there is probably little to choose between the two methods, although, if anything, the second method is perhaps capable of the greater refinement.

§ 6. ACKNOWLEDGMENT

We are much indebted to Mr Y. S. Chiong for help with the calculations.

* See Andrade, *Phil. mag.* **17**, 708 (1934).

A METHOD OF INVESTIGATING THE HALL EFFECT

By H. STAFFORD HATFIELD, Ph.D.

ABSTRACT. A cylindrical coil of thin metal strip of width equal to the length of the coil is connected to a galvanometer and set between the poles of a magnet rotating about the axis of the coil, the field being at right angles to the latter direction; or a stationary three-pole magnet fed with three-phase current may be used. The galvanometer indicates a steady direct e.m.f. due to the sum of two Hall e.m.fs. per turn of the coil, resulting from the action of the field upon the currents induced in the coil by the rotation of the field. The e.m.fs. are much larger than those obtainable by ordinary methods, and there are no thermoelectric or other disturbing effects. An approximate formula is derived, and is shown to agree roughly in the case of seven metals with data taken from tables. It is suggested that the method might be of use for comparative work at extreme conditions.

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§ 1. OUTLINE OF THE METHOD

THE usual method for measuring the Hall effect consists in placing between the poles of an electromagnet a strip of the metal under examination and passing through it a primary current as large as possible and usually direct. At two points at opposite ends of an equipotential line on the strip electrodes connected to a sensitive galvanometer are applied, so that no current passes through them when the magnet is unexcited and the electrodes are truly placed. When the magnet is excited, the galvanometer indicates the presence at the electrodes of an e.m.f. E_H given by

$$E_H = \frac{RHi}{d},$$

where H is the field, i the primary current, d the thickness of the strip in the direction of the magnetic field, and R the Hall coefficient. For most metals R in absolute units is of the order of 10^{-3} and for the magnetic metals of 10^{-2}, while for antimony it is 10^{-1}, for bismuth from 1 to 10, and for tellurium 500. Possible values of i are limited by the specific resistances of the metals, which, very roughly speaking, follow the Hall coefficient in magnitude. Hence the larger the R to be measured, the smaller is the current which can be used for measuring it. The practical outcome is that Hall coefficients are determined by the measurement of a very few microvolts, and usually on thin specimens the thickness of which enters directly into the formula. The small transverse e.m.f. is measured in the presence of the large primary current and its corresponding potential fall.

E_H

H, i, d
R

18-2

The obvious disadvantages attendant on this arrangement are mitigated to some extent by the use of alternating primary current and an alternating field, but the smallness of the measured e.m.f. remains, and no advantageous method of increasing it by summation of a number of sources has yet been devised.

The purpose of the present paper is to describe a method of exhibiting the Hall effect based upon the principle of generating the primary current by the inductive action of a rotating magnetic field upon a cylindrical sheet of the metal set coaxially with the axis of rotation of the field. The Hall effect then arises from the action of the same magnetic field upon the currents so induced. This method has the advantage of dispensing altogether with delicately adjusted Hall electrodes, and the further advantage, if a cylindrical coil of sheet metal of many turns is used, of allowing a large number of Hall e.m.fs. to act in series. In these experiments,
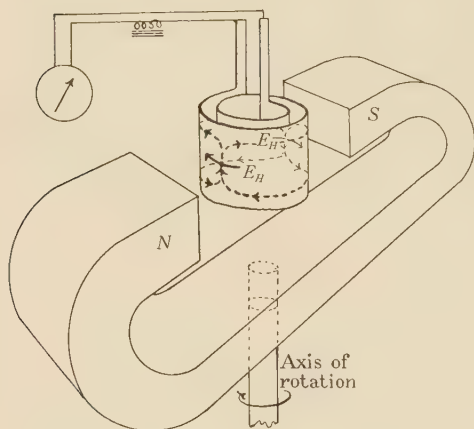
Figure 1.

Figure 2.

Figure 1. Generation of Hall e.m.f. E_H in a strip metal coil exposed to the field of a rotating magnet, the Hall coefficient being positive.
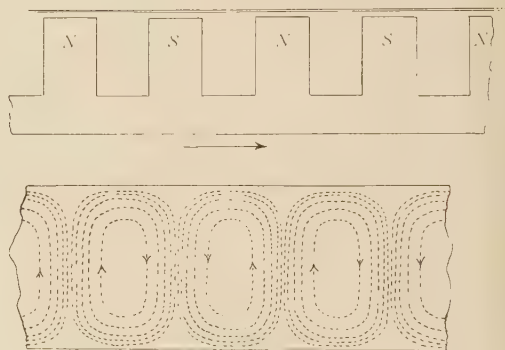
Figure 2. Diagram of current eddies in strip exposed to field of moving magnetic poles.

metals with a low coefficient were made to deliver a Hall e.m.f. exceeding 100 μV. in fields of only 3000 G., and it would be easy to multiply this many times.

The general principle of the method will be clear from figure 1. A long strip of the metal under investigation is wound into a close spiral and exposed to a magnetic field at right angles to the axis of the spiral and rotating about it. A system of induced currents is set up in the spiral as indicated by the dotted lines in the figure, and the action of the field on these currents generates Hall e.m.fs. The Hall e.m.f. generated in each current-path parallel to the axis is in the same sense with reference to the axis, since the directions of both current and field are opposite on opposite sides of the spiral. The Hall e.m.fs. therefore add up in each turn, and from turn to turn. Figure 2 makes this clearer by exhibiting the strip as unrolled and exposed to the fields of a series of alternate magnetic poles moving beneath it in the direction of its length, the induced current eddies being suggested by the dotted lines.

We shall in the next section describe the results of measurements of the e.m.f. under different conditions and in different metals, and give the experimental evidence that this e.m.f. is connected simply with the Hall effect, reserving for § 3 a discussion of the system of induced currents, and the theoretical reasoning which supports the above conclusion.

§ 2. EXPERIMENTAL DETAILS

We will first describe the experiments carried out with copper sheet 0·05 mm. thick. A strip of this, 19·6 mm. wide, was wound upon a brass rod 3 mm. in diameter to form a coil of 16 turns, which were insulated from one another by paper tape. The magnet used was a specially designed magnet with an iron circuit, shaped as a rectangle with a central crossbar (i.e. two rectangles having one side in common), and set with the pole axis horizontal upon a turntable having a vertical axis. This form of magnet is especially suitable for the purpose, since the centre of gravity is low. The maximum attainable field was 7000 G. in a space 1 cm. long and 2·5 cm. in diameter, the copper coil being set in the centre of this with its axis vertical. The maximum safe speed of rotation was 600 r.p.m. An ordinary counter was used to measure it.

The strip coil was connected through a 20-Henry wireless choke to a galvanometer of 100 ohms resistance, the total resistance in circuit being about 300 ohms. The sensitivity of the arrangement was found to be 15·1 μV./cm. on the scale. The choke is necessary, since the inevitable departure from symmetry of the arrangement leads to the appearance at the terminals of the coil of an alternating e.m.f. It is clear that a much more powerful magnet, and higher speeds of rotation, could be used if the coil instead of the magnet were rotated, but this would involve slipping contacts in the galvanometer circuit.

Measurements were made of the deflection of the galvanometer for various speeds of rotation and various values of the magnetic field. If the deflections are in fact due to the Hall effect and therefore dependent upon the product of the magnitude of the induced currents and the field to which they are exposed, then, in accordance with § 3, we should expect the galvanometer deflections to increase linearly with the speed of rotation but according to the square of the magnetic field. Figure 3 shows the relation between observed microvolts and speed of rotation, and it will be seen that within the accuracy of the observations the dependence is approximately linear. This proves no more than that the effect is due in some way to induced currents.

Figure 4 shows the dependence of the direct e.m.f. upon the magnetic field-strength, and there is good evidence for a linear variation with H^2. This suggests very strongly that we are here concerned with an action of the magnetic field upon the induced currents, which themselves depend upon field-strength.

The final proof of the Hall origin of the effect was then obtained by performing experiments with different metals. These were carried out by means of a specially constructed three-pole magnet fed with three-phase 50-cycle current from the mains. The pole-gap was cylindrical (like the pole-tunnel of an induction motor)

being 12 mm. in diameter and 20 mm. long. The pole-surfaces occupied about half the periphery. A field of 3000 G. could be maintained continuously, and 4100 could be obtained on overload. The average value of the stray field over a distance at either end along the axis of the pole-tunnel and equal to its length was about one-half of the above.
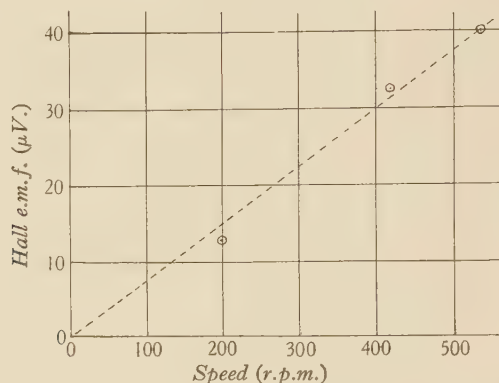
Figure 3. Relation between Hall e.m.f. and speed of rotation of field.
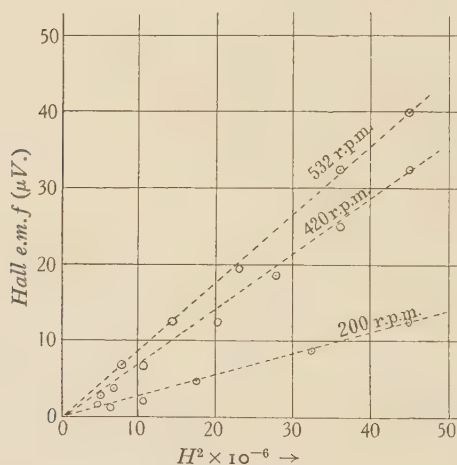
Figure 4. Relation between Hall e.m.f. and square of strength of rotating magnetic field.

Experiments were first made with the 0·05-mm. copper sheet to determine the effect of the width of the strip upon the e.m.f. developed. It will be seen from figure 5 that there is an optimum value for the width, as might be expected. When it much exceeds the axial length of the field space, the Hall current can take a short-circuit path along its edges. On the other hand, as the width of the strip is diminished the induced e.m.f. is diminished in like proportion, while the ohmic resistance to the induced currents diminishes much more slowly, and finally increases. The general shape of the curve is thus accounted for. For the experiments which follow, a strip width of 25 mm. was chosen.

The table shows the observed values in absolute units of the e.m.fs. generated in coils of this width, but of various lengths and thicknesses of strip, in the three-phase field of 3000 G., as compared with the Hall coefficients and specific resistances of the metals taken from tables.*

Figure 5. Hall e.m.f. delivered by coils of copper strip differing only in width.

Table

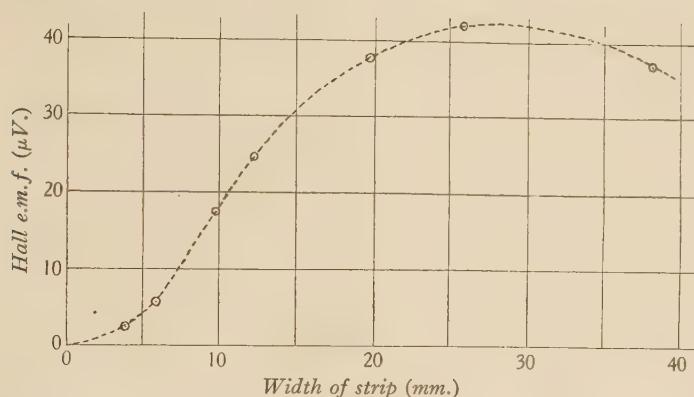| Metal	E.m.f. observed (absolute e.m.u.) E_H	Hall coefficient (absolute e.m.u.) R	Specific resistance (absolute e.m.u.) σ	Length (cm.) L	Thickness (cm.)	Turns n	$\dfrac{4E_H \sigma n}{RL^2}$ $\times 10^{-8}$
Aluminium	− 1,350	− 0·0004	2,820	55	0·0025	23	2·9
Copper	− 4,077	− 0·0005	1,720	60	0·0078	20	3·1
Lead	+ ca. 50	+ 0·0001	20,630	35·3	0·02	14	4·7
Nickel	+ 33,900	+ 0·012	7,240	87·5	0·0045	34	3·6
Silver	− 14,900	− 0·0008	1,620	122	0·0025	48	3·9
Zinc	+ 1,440	+ 0·001	5,920	62	0·0097	23	2·0
Tin	0	− 0·00002	11,400	100	0·0025	45	—

The signs attached to E_H in the second column are chosen arbitrarily so that a metal having a positive Hall coefficient should, if the scheme of figure 1 is correct, give rise to a positive E_H, and *vice versa*. A positive Hall coefficient gives rise to a current through the coil and galvanometer flowing through the former in the same conventional direction as the rotation of the field. The simple electron theory of the Hall effect predicts a negative coefficient, and in this case the *electron* current is in the same direction as the rotation of the field.

It will be shown in the next section that if the e.m.f. is due only to the Hall effect, the figures in the last column should be constant and of the order 4.5×10^8. In view of the very rough character of the calculation, the resulting agreement is very satisfactory. It may be noted in passing that Sommerfeld and Frank[1] say that "the Hall effect can be relied upon on the average with a precision of some 30 per cent".

* *R* from Campbell, *Galvanomagnetic and Thermomagnetic Effects* (London, 1923) and σ from Childs, *Physical Constants* (London, 1934).

The conspicuous absence of bismuth from the table is, of course, due to the fact that the construction of strip coils of many turns would be difficult on account of its brittleness. Taking a value $3\cdot0 \times 10^8$ for the last column, R for bismuth as $6\cdot0$, σ as $119,000$, and $L = 2n$, we calculate E_H/n to be $150\,\mu V.$ *per turn*. A bismuth coil should thus be capable of yielding much larger voltages than those measured above.

The absence of the effect in mercury is well known, and an effort was made to determine it by the present method, which presents great advantages for liquid metals in view of the avoidance of the well-known disturbing effects arising from the flow of the metal under the electrodynamic forces. A mercury coil was made as follows. A strip of paper 35 mm. wide and two strips 5 mm. wide were saturated with paraffin wax. A glass tube of external diameter 4 mm. was closed at one end, and a hole was blown in the side of it 1 cm. from the closed end. The wide and the two narrow strips were then wound together on the tube at the closed end, the narrow strips being set along each end of the wide strip, so as to space apart the convolutions of the wide strip by the thickness of the narrow strip. The coil was then thrust into a test tube at the bottom of which was a little molten wax, and some wax was also melted over the coil at the upper end by means of a hot glass rod, care being taken to leave a space at one point between the coil and the glass. Finally, mercury was poured down the centre tube; it filled the space between the convolutions of the paper, so forming a continuous coil of the size and shape of the strip coils used in the other experiments. It had 20 turns. It was thus comparable with the copper coil, which gave an e.m.f. of about $40\,\mu V.$; if its Hall coefficient were the same as that of copper, it might be expected to give about $1\cdot8\,\mu V.$, since its specific resistance is much higher. Not the slightest effect on the galvanometer could be observed, though less than $0\cdot5\,\mu V.$ could certainly have been detected. The experiment is only valuable as an illustration of the ease with which spurious effects can be eliminated by the present method.

The summation of the e.m.f. along the strip was confirmed by providing a copper strip coil 60 cm. long with tappings every 10 cm. The coil had 20 turns, but the tappings were of course not equally distributed as regards these, since the internal diameter of the coil was 3 mm. and its external diameter was 10 mm. Nevertheless, the e.m.f. from tapping to tapping was almost identical within the limits of error of the readings. The reason for this is seen in the light of the discussion given in the next section.

The induced currents are considerable, and an estimate of their magnitude can be derived from the following experiment on their heating effect. The silver coil referred to in the table was placed in the three-phase field, and its rise of temperature was determined by measuring its increase in ohmic resistance; it was found to have increased by $19°$ in 3 minutes. Now, for a metal of specific resistance σ, specific heat C, and density δ, the rate of increase in temperature due to current density u is, when losses are neglected, given by the equation

$$\frac{dT}{dt} = \frac{u^2\sigma}{C\delta}.$$

Taking $\sigma = 1{\cdot}64 \times 10^{-6}$, $C = 0{\cdot}056 \times 4{\cdot}2 \times 10^7$, and $\delta = 10{\cdot}5$ we find for u the value 398 A./cm².

This is the r.-m.-s. value for the whole strip. The currents do not occupy more than half the area of the strip, so we may safely double the above value, and make it say 800 A./cm². The strip being 0·0025 cm. thick, and the current paths on the average about 1 cm. wide, the currents must be of the order of 2 A. From this experiment a further important result will be obtained in the next section.

§ 3. DISCUSSION OF RESULTS

The following discussion makes no claim to completeness or exactness. It is justified by the fact that with the simple formulae obtained the experimental results can be calculated almost within the limits of error of the present experiments, and of the physical constants used.

The case resembles that of the well-known squirrel-cage alternating-current induction motor with its armature held stationary; there, however, the induced currents are forced into well-defined paths by the construction of the rotor. In the present case a calculation of the distribution of the induced current obviously presents great difficulties; even if an approximate solution were found for complete turns, the discontinuities introduced at the two ends of the coil would introduce further difficulties.

As in the squirrel-cage motor, the induced currents must lag behind the inducing field by an amount given by $\cos \phi = W/\sqrt{(W^2 + 4\pi^2 f^2 L^2)}$, where W typifies the resistance of the current path, L its inductance, and f the frequency. In the present arrangement, a further complication is introduced by the fact that the inner turns are shielded by the outer turns, and hence the phase angle may differ from turn to turn.   W
L, f

It will be seen that, in general, any calculation which neglects the phase angle will give too large a result, since the Hall intensity affecting the galvanometer is directed at right angles to the field, and this is directed at an angle ϕ to the strip at the point where the induced current is flowing.   ϕ

However, leaving that point out of consideration for the moment, we can easily find from dimensional arguments that the Hall e.m.f. E_H will be given by an expression

$$E_H = RH^2 \, \Phi \left(\frac{f}{\sigma} L^2 \right) \qquad \ldots\ldots(1),$$

Φ

where L^2 denotes some homogeneous expression of the second degree of the dimensions of the spiral coil. In order to obtain further details about the form of the function Φ, and information as to the manner in which the various dimensions of the coil enter into L^2, we suggest the following considerations based on approximation to the actual conditions.

Consider a metal foil in the y, z plane, exposed to a magnetic field H_x in the x direction. If H_x varies, we have as a first approximation in any one spot   H_x

$$\sigma \left(\frac{u_z}{y} - \frac{u_y}{z} \right) = \frac{dH_x}{dt} \qquad \ldots\ldots(2),$$

where σ is the specific resistance, u_z current density in the z direction, and u_y that in the y direction.

The Hall intensity I measured on the galvanometer is due to the component in the direction y, and in general

$$I_y = R\,[\bar{u}_z . \bar{H}_x]\qquad\ldots\ldots(3).$$

There is also an I_z equal to $R\,[\bar{u}_y . \bar{H}_x]$, but this does not affect the galvanometer.

On the centre line of the strip, and at a point near to each pole, u_y will be small but u_z large. Here will arise the main external Hall e.m.f. Very approximately we may write

$$u_z \sim \frac{1}{\sigma}\int \frac{dH_x}{dt}\,dy\qquad\ldots\ldots(4).$$

The time factor can only introduce the frequency of rotation f into the numerator, hence u_z will depend mainly upon a quantity $\int H_x dy$, that is to say, on the magnetic field integrated in a horizontal direction perpendicular to H.

In order to obtain the Hall e.m.f. from the Hall intensity, we have to perform an integration right along the spiral length; thus by inserting equation (4) in equation (3) we obtain

$$\text{e.m.f.} = R\int^{\text{Whole length}} Iy\,dy$$

$$= \frac{Rf}{\sigma}\int^{\text{Whole length of foil}} H\int^{\text{Breadth of field}} H\,dy\,dy\qquad\ldots\ldots(5).$$

As a very rough approximation, we may obtain the order of magnitude of these integrals by equating them to

$H^2 \times$ (total length of foil exposed to magnetic field)
$\times$ (average length exposed to field in any half turn).

Each of these lengths may be taken very approximately as one-half of the actual length of the strip.

Finally, we get for the n turns of the coil, made of strip having an actual length L,

$$E_H = KfH^2\frac{RL^2}{4n\sigma}\qquad\ldots\ldots(6),$$

where K is a constant of the order of unity.

fH^2 was constant throughout the experiments, hence $4E_H n\sigma/RL^2$ should be constant. It will be seen from the table that this was remarkably near to being the case. Still more surprising, in view of the very rough derivation of the formula, is the close approximation of these values to fH^2, which was 4.5×10^8. We even see that the actual values are on the average somewhat smaller than this, as is to be expected in view of the neglect of the phase angle.

As a further argument in support of the view we have put forward, we may now consider the experiment, described in the last section, in which the rate of rise of temperature of the silver coil was measured. The Hall intensity is given by $R\,[\bar{u}.\bar{H}]$, where u is the current density at a point where the field is H. In the preceding section, the average value of u was obtained from the relation $u^2 = C\delta dT/\sigma dt$.

Since the final potential-difference is due to the intensity integrated along the length of the strip, we may write

$$E_H = RHL \, \frac{C\delta dT}{\sigma dt} \qquad \qquad \dots \dots (7),$$

where L is the actual length of the strip, since it was all heated, and the value of u was obtained from its rise in temperature. Using the values given in the table, and the rise of temperature as given in the preceding section, we find for E_H the value $150 \, \mu$V.; the observed value was $149 \, \mu$V. The close agreement is, of course, quite fortuitous, but it clearly proves that the above treatment of the phenomenon cannot be very far wrong.

§ 4. CONCLUSION

The foregoing experiments and discussion appear to establish the fact that the e.m.f. measured is entirely due to the Hall effect. The effect is comparatively large and hence easy to measure with accuracy, and no spurious or disturbing effects are found to occur, the readings of the galvanometer being in all cases extremely steady and reproducible. While the absolute values as derived from the formulae given are obviously uncertain in view of the very rough derivation of these formulae, it would appear that comparative measurements would yield results of considerable accuracy, especially if the specimens used were alike in all dimensions. The discussion suggests that it might be best to keep f/σ and not f constant, in which case the relative values of R should be independent of phase relations. Since the e.m.f. measured is the sum of a large number of single e.m.fs., the unavoidable variations in the thickness of the specimen tend to average out. The thickness enters into the formula in the value of σ, but its determination on the actual specimen is more satisfactory than in the usual method, since a much longer strip is used.

If a suitable container could be constructed of rigid and permanent insulating material capable of standing elevated temperatures, it is probable that the method would present great advantages for liquid metals. At low temperatures also it should be advantageous when measurements on single crystals are not required.

§ 5. ACKNOWLEDGMENTS

Note added January 4, 1936

When this paper was read, I pointed out that the observed direct current produced by the rotation of the magnet, though readily explicable by combining the law of electromagnetic induction with the known facts of the Hall effect, seemed to conflict with the usual conception of electromagnetic induction. If a fixed homogeneous

metallic circuit is exposed to a cyclically varying magnetic field, the constants of the circuit remaining unchanged throughout the cycle of the field, the flow of electricity resulting from increase in the flux linked with the circuit should be equal and opposite to that resulting from decrease, and the generation of a balance of unidirectional current should be impossible. This argument was often used against the innumerable attempts by inventors to effect such a result in the early days of dynamo development.

A further remark may be of interest. If we replace the strip coil of figure 1 by a simple hollow cylinder, we must say that the induced currents flow in a direction not at right angles to the field and to its motion, but slightly inclined one way or the other. This must also be true in the homopolar ("unipolar") dynamo, where a copper tube rotates in a concentric pole gap like that of a loud-speaker magnet. If instead of rotating the tube, we apply an external source of e.m.f. to its ends, the fall of potential for a given current will be increased when the magnet is excited, for a Hall current will flow peripherally around the tube. As Bridgman showed[2] the energy of a Hall current is derived from an increase in potential-fall in the primary circuit. Making use of this, it is easy to show that the resistance ρ of the part of the tube exposed to the field will apparently increase by an amount $\Delta\rho$ given by

$$\frac{\Delta\rho}{\rho} = \frac{R^2 H^2}{\sigma^2}.$$

In this case the direction of primary current-flow is undoubtedly changed from axial to spiral, and is presumably inclined to the acting e.m.f. The primary current is forced by the field to take a longer path of increased resistance, and the Hall current is simply the peripheral component of the primary current. This picture of the Hall effect seems to me physically simpler and more fundamental than the usual conception, concerning which much confusion has in fact arisen, as Campbell points out. However, ordinary magneto-resistance introduces a complication, for it would lead in most cases to an increase greater than that due to the Hall effect, as the following table shows. The data for R and σ are those given in the paper, and the magneto-resistance coefficients C (of H^2) are taken from *International Critical Tables*:

Metal	R^2/σ^2	C
Copper	$8\cdot5 \times 10^{-14}$	$2\cdot7 \times 10^{-13}$
Lead	$2\cdot35 \times 10^{-17}$	$5\cdot2 \times 10^{-14}$
Silver	$2\cdot45 \times 10^{-13}$	$2\cdot5 \times 10^{-13}$
		$3\cdot7 \times 10^{-13}$
Zinc	$2\cdot9 \times 10^{-14}$	$8\cdot5 \times 10^{-13}$
		$1\cdot4 \times 10^{-12}$
Tin	$3\cdot05 \times 10^{-18}$	$2\cdot3 \times 10^{-13}$

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REGULARITIES IN THE SPECTRUM OF TREBLY IONIZED IODINE

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ABSTRACT. The spectrum of a condensed discharge through iodine has been photographed in the region λ 6000 to λ 1900 under varying conditions of excitation, and about 70 lines have been ascribed to trebly ionized iodine. Among these more than 30 lines have been classified; assignments for the levels have been suggested on the analogy of the other isoelectronic spectra, Te III, Sb II and Sn I.

§ 1. INTRODUCTION

AFTER the analysis of Te III carried out in this laboratory[1], an investigation of the spectrum of trebly ionized iodine I IV was undertaken. The spark spectrum of iodine has been studied by Wood and Kimura[2], Konen[3], Exner and Haschek[4], Asagoe[5], L. and E. Bloch[6], and Kerris[7], but an analysis of the spectra due to the higher ionized stages of the atom has not been attempted by any one of the above investigators. Recently, Lacroute[8] studied the Zeeman effects in some low-stage iodine lines and published an analysis of I II. Our knowledge of the structure of Te III made possible the prediction of the probable values of the intervals and the positions of the characteristic lines in the spectrum, and facilitated the discovery of several regularities among the lines due to I IV. It is the purpose of this paper to report these regularities.

§ 2. EXPERIMENTAL

In the experiments of L. and E. Bloch, the spectrum of I IV appears to have been excited very feebly, the lines recorded by them being faint. A reinvestigation was made by the writer, in which the spectrum was excited much more intensely. The source used was a highly condensed discharge through a capillary tube. Pure iodine, or in some cases iodide of copper supplied by Kahlbaum, was placed in a side bulb of the discharge tube. After the tube had been worked for some time by periodical gentle heating of the bulb, the heat of the discharge was sufficient to keep the capillary filled with iodine vapour. The discharge was run by a $\frac{1}{2}$-kW. transformer and condensed by a capacity of about $0·05\mu F$. An auxiliary spark gap and a self-inductance used in series with the discharge served, as usual, to alter the degree of excitation. While the discharge was running, a deposit of iodine was formed on the quartz window of the discharge tube, which necessitated frequent cleaning and refixing of the window during the experiment. The exposures extended from two to three hours. Ilford special rapid plates were employed, and for the ultra-violet between λ 2400 and λ 1900 the plates were sensitized by paraffin oil. As, however,

these sensitized plates were found unsuitable for measurement, Schumann plates prepared by the writer after the manner described by Schumann[9] were used. The instruments employed were a Hilger quartz spectrograph E 315 for the region λ 5000 to λ 1900, and a glass Littrow spectrograph for the entire visible region.

§ 3. ANALYSIS

The spectrum of I IV resembles that of Te III, and a brief statement of the important terms predicted by Hund's theory is presented in table 1.

Table 1

Term prefix	Terms
6s	^{3}P; $\quad$ ^{1}P.
5d	^{3}F, ^{3}D, ^{3}P; ^{1}F, ^{1}D, ^{1}P.
6p	^{3}D, ^{3}P, ^{3}S; ^{1}D, ^{1}P, ^{1}S.

The number of lines attributable to I IV are comparatively few, so that, although the characteristic intervals are large, the selection of multiplets was not found difficult. By extrapolation from the known values of the interval $6s\ ^3P_1$–$6s\ ^3P_2$ in the isoelectronic spectra Sn I, Sb II, Te III, the probable value of the corresponding interval in I IV could be arrived at. The regular diminution in the values of the screening constant with increasing nuclear charge in these spectra is shown in table 2. The value finally obtained in I IV is also included for comparison.

Table 2. The law of screening constants

Spectrum	Interval $6s\ ^3P_1$–$6s\ ^3P_2$	Screening constant	Difference
Sn I	3715	27·1	
Sb II	5739	24·46	2·64
Te III	7698	22·52	1·94
I IV	8532	21·8	0·72

The approximate position of the combination $6s\ ^3P_2$–$6p\ ^3P_2$ could be predicted from the sequence presented in table 3. A search for recurring pairs in the region below λ 4000 revealed three pairs having the fundamental difference 8532 cm.$^{-1}$

Table 3. The law of irregular doublets

Spectrum	$6s\ ^3P_2$–$6p\ ^3P_2$	Difference
Sn I	8605	
Sb II	16448	7838
Te III	24248	7805
I IV	31110	6862

When the combination $6s\ ^3P_2$–$6p\ ^3P_2$ was thus identified it was easy to extend the analysis to include the other $6p$ term-combinations.

The results are presented in multiplet form in table 4, which includes regularities among only those lines which could be confidently ascribed by the writer to I IV; lines of doubtful assignment are provisionally excluded. An analysis of the lines of I III, which is also being carried out by the writer, will decide their classi-

Table 4. Terms and multiplets of I IV

Odd terms \ Even terms		a $6p\ ^3D_1$ 158739·4	b $6p\ ^3D_2$ 157826·3	c $6p\ ^3D_3$ 147555·7	d $6p\ ^1D_2$ 159135·2
		913·1	10270·6		
A $6s\ ^3P_0$	201079·0	42339·6 (7)			
B $6s\ ^3P_1$	200532·8 546·2 8532·8	41793·1 (6)	42706·5 (4)		41397·8 (0)
C $6s\ ^3P_2$	192000·0	33260·8 (1)		44444·3 (5)	
D $6s\ ^1P_1$	200340·8	41601·1 (4)			41205·9 (9)
E $5d\ ^3F_2$	201113·7	42373·6 (2)	43288·3 (3)		
F $5d\ ^3D_1$	199981·7	41243·4 (3)	42155·2 (3)		
G	201272·3	42532·5 (1)			42137·6 (6)
H	202768·3		44941·3 (3)		
J	198423·1		40597·5 (3)		

Odd terms \ Even terms		e $6p\ ^3P_0$ 172347·4	f $6p\ ^3P_1$ 160857·9	g $6p\ ^3P_0$ 160889·7	h $6p\ ^1P_1$ 169691·8	i 160585·6
		11489·5	-31·8			
$6s\ ^3P_0$	201079·0		40221·0 (4)			
$6s\ ^3P_1$	200532·8 546·2 8532·8	28185·6 (4)	39674·7 (8)	39643·5 (6)		
$6s\ ^3P_2$	192000·0		31142·5 (4)	31109·9 (6)		
$6s\ ^1P_1$	200340·8		39482·9 (4)			
$5d\ ^3F_2$	201113·7		40255·3 (0)		31421·3 (4)	40528·0 (3)
$5d\ ^3D_1$	199981·7				30290·3 (3)	39396·4 (5)
	202768·3		41910·9 (4)	41878·9 (8)		
	204954·8		40097·0 (4)	40065·8 (9)		
	198423·1				28731·5 (2)	37837·2 (5)

fication; these will be presented in a future paper. The terms are designated arbitrarily as $A, B, C, D, \ldots$ when they are supposed to be odd, or as $a, b, c, d, \ldots$ when they are even. The suggested assignment for these terms also is given at the left and top of the table. In assigning these levels, greater consideration was paid to a comparison with the corresponding levels in other isoelectronic spectra than to the

indication of the intensities of the lines in the various levels, for, on account of the expected large deviations from the normal *LS* coupling, the interval and intensity rules are liable to be departed from to a very great extent.

One peculiar feature according to the above assignment of the terms is that $6p\ ^3P$ is partially inverted, as is observed also in Te III, but not in Sn I or Sb II. The term $6p\ ^3D$ is, however, remarkably regular in the whole row of these spectra, and it might be significant to notice that this term is, according to Hund, the deepest triplet term arising from the $6p$ configuration.

Table 5. Lines of I IV

λ	ν	Classification	λ	ν	Classification	λ	ν	Classification
3546·90 (4)	28185·6	B–e	2545·67 (8)	39270·6		2418·82 (3)	41329·9	
3479·51 (2)	28731·5	J–h	2537·54 (5)	39396·4	F–i	2418·45 (2)	41336·2	
3300·44 (3)	30290·3	F–h	2531·98 (4)	39482·9	D–f	2414·85 (0)	41397·8	B–d
3224·90 (6)	30999·8		2521·72 (6)	39643·5	B–g	2403·61 (3)	41591·4	
3213·48 (6)	31109·9	C–g	2519·74 (8)	39674·7	B–f	2403·05 (4)	41601·1	D–a
3210·12 (4)	31142·5	C–f	2516·82 (2)	39720·7		2392·00 (6)	41793·3	B–a
3181·64 (4)	31421·3	E–h	2513·74 (3)	39769·4		2387·11 (8)	41878·9	H–g
3170·11 (4)	31535·6		2511·04 (3)	39802·1		2385·28 (4)	41910·9	H–f
3153·86 (3)	31698·1		2501·38 (2)	39965·9		2376·46 (7)	42066·5	
3151·61 (1)	31720·7		2496·05 (3)	40051·2		2372·45 (6)	42137·6	G–d
3069·17 (4)	32572·6		2495·14 (2)	40065·8	I–g	2371·46 (3)	42155·2	F–b
3005·67 (1)	33260·8	C–a	2493·20 (4)	40097·0	I–f	2367·75 (5)	42221·3	
2931·11 (3)	34118·8		2489·24 (5)	40160·8		2361·13 (7)	42339·6	A–a
2917·33 (5)	34267·9		2485·51 (4)	40221·0	A–f	2359·24 (2)	42373·6	E–a
2910·97 (4)	34342·8		2483·39 (0)	40255·3	E–f	2353·45 (1)	42477·8	
2885·15 (3)	34650·1		2475·71 (0)	40380·2		2350·42 (1)	42532·5	G–a
2864·68 (6)	34897·7		2475·35 (8)	40386·1		2340·84 (4)	42706·5	B–b
2839·42 (2)	35208·1		2466·96 (3)	40523·5		2309·38 (3)	43288·3	E–b
2818·45 (3)	35470·1		2466·68 (3)	40528·0	E–i	2280·05 (2)	43845·1	
2736·64 (1)	36530·3		2462·46 (3)	40597·5	J–b	2249·30 (5)	44444·3	C–c
2652·23 (8)	37692·9		2434·85 (6)	41057·8		2238·11 (1)	44666·6	
2642·11 (5)	37837·2	J–i	2426·10 (9)	41205·9	D–d	2224·43 (3)	44941·3	H–b
2640·77 (4)	37856·4		2423·89 (3)	41243·4	F–a			

Further, if the identification of $6s\ ^1P_1$ is correct, it is observed to be deeper than $6s\ ^3P_2$. This indicates a tendency to approach, together with the terms $6s\ ^3P_1$ and $6s\ ^3P_0$, to the limit $5p\ ^2P_{\frac{1}{2}}$ of the next higher ion; there is thus an intermingling of the triplet and the singlet terms of the $6s$ configuration.

Two intense lines, ν 40386·1(8) and ν 44444·3(5) might be suggested as probably forming the combinations $5d\ ^3F_4$–$6p\ ^3D_3$ and $6s\ ^3P_2$–$6p\ ^3D_3$.

The term-values given in table 4 are based on the value 192000 assumed for the term $6s\ ^3P_2$, and obtained by extrapolation from the corresponding values in Sn I, Sb II, Te III; this value might serve at least as well as one arbitrarily chosen.

Finally, a list of the lines due to the spectrum of I IV, along with the classification suggested for some of these in the present work, is given in table 5.
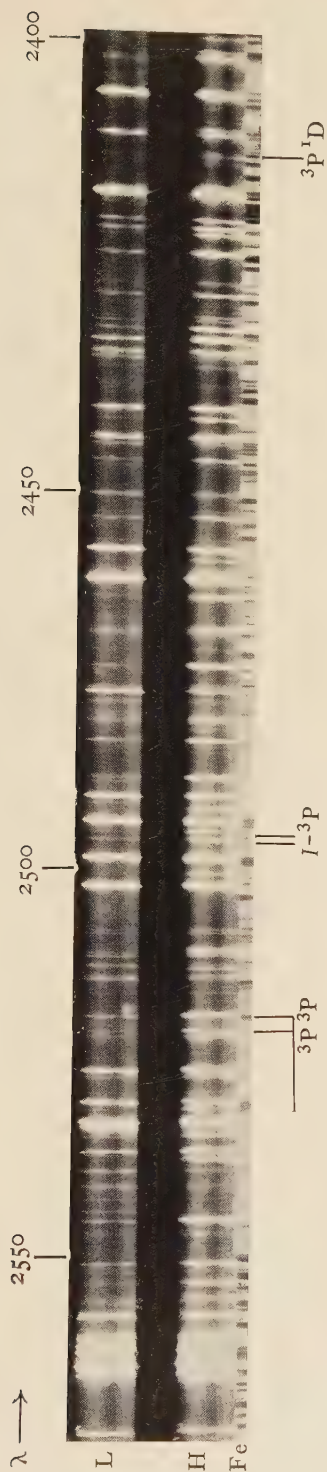
L, Discharge-tube spectrum of iodine under lower excitation. H, Discharge-tube spectrum of iodine under higher excitation. Fe, Spectrum of the iron arc for comparison.

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THE EFFECT OF DISSOLVED AIR ON THE SPECIFIC HEAT OF WATER OVER THE RANGE 15° TO 20°C.

By E. O. HERCUS, D.Sc., F.Inst.P., Associate-professor in Natural Philosophy, University of Melbourne

ABSTRACT. The effect of dissolved air on the specific heat of water is calculated thermodynamically from the solubility data, for the case when no air is liberated, either completely or in the form of bubbles. It is shown that the specific heat of water saturated with air at 20° C. under normal atmospheric pressure exceeds that of air-free water by 6×10^{-6} cal./g.

§ 1. INTRODUCTION

IN a recent paper by Laby and Hercus[1] it was shown that the liberation of air from water on heating could have no significant effect on the specific heat. Dr Jessel, in the discussion, suggested that the air might affect the specific heat without being liberated. In reply it was stated, on thermodynamic grounds, that the difference could not exceed 1 in 100,000. This paper gives a detailed statement of the grounds for that conclusion.

§ 2. THE HEAT OF SOLUTION OF AIR IN WATER

L The molar latent heat L of evaporation of air from a solution in water to air in equilibrium with it is given by:

$$L = RT^2 . \partial \log K / \partial T,$$

K where K is the constant of Henry's law given by $p_A = Kx$, x being the mol-fraction of air dissolved under a pressure of p_A cm. of mercury. From the values of K given in the table[2]

Temperature (°C.)	$10^{-7} \times K$	Temperature (°C.)	$10^{-7} \times K$
14	4·525	19	4·964
15	4·612	20	5·044
16	4·701	21	5·130

we have $L_{15} = 3150$ cal./mole, $L_{20} = 2810$ cal./mole.

These values include the work done against the external pressure; by subtracting

L' RT we obtain the corresponding changes L' in internal energy:

$$L'_{15} = 2570 \text{ cal./mole}, \qquad L'_{20} = 2230 \text{ cal./mole.}$$

§3. COMPARISON OF SPECIFIC HEATS

By consideration of the following cycle it is possible to compare the specific heat, S_2 international cal./g.,* of water containing enough air to saturate it at 20° C. under normal atmospheric pressure, with the specific heat, S_3 international cal./g. of air-free water. Taking one gram of water, the amount n of air involved is, from the table above quoted, $8 \cdot 4 \times 10^{-8}$ mole.

(a) Cool the aerated water from 20° C. to 15° C. The loss of entropy from the system is $\int_{288}^{293} \frac{S_2}{T} dT$.

(b) Release the pressure isothermally from normal atmospheric pressure π to the pressure p_1 at which the solution is saturated at 15° C. Here $\pi/p_1 = 1 \cdot 094$. The gain in entropy B in this stage is calculable from the Maxwell relation

$$\left(\frac{\partial \phi}{\partial p}\right)_T = -\left(\frac{\partial v}{\partial T}\right)_p,$$

but it is offset by a corresponding gain later since, as Chappuis's experiments[3] have shown, the density of water is not appreciably affected by dissolved air.

(c) Suppose the water surface to be provided with a semi-permeable membrane freely permeable to air but not to water vapour. By withdrawing a piston from the surface, allow the air to come out of solution until, when the pressure is reduced from p_1 to p_0, a negligible amount of air remains in solution; p_0 disappears in the final equations and so may be arbitrarily small. If this is carried out isothermally, the work done by the air in expanding to volume v_0 is $\int_0^{v_0} p\, dv$. At any stage in this process, out of the n moles of air an amount proportional to p, say kp, is in solution and the remainder, $n - kp$, occupies as a gas a volume v under pressure p.

Hence
$$pv = (n - kp)\, RT,$$

$$p = \frac{nRT}{v + kRT},$$

$$\int_0^{v_0} p\, dv = nRT \log_\epsilon \frac{v_0 + kRT}{kRT} = nRT \log_\epsilon \frac{p_1}{p_0}.$$

The gain in entropy in this stage is therefore

$$n \frac{L'_{15}}{288} + nR \log_\epsilon \frac{p_1}{p_0}.$$

(d) Separate the water from the air. Increase the pressure on the water from p_1 to π, isothermally, with a loss of entropy B—the inverse of stage (b). Then heat the air-free water to 20° C. Heat the air under pressure p_0 to 20° C. The entropy gained is

$$-B + \int_{288}^{293} \frac{7 \cdot 02n}{T} dT + \int_{288}^{293} \frac{S_3}{T} dT.$$

* It is most convenient to define the other heat quantities in terms of the 15° C. calorie.

19-2

(e) Compress the air isothermally from pressure p_0 to the pressure p_0' at which it is in equilibrium with the infinitesimal amount of air remaining in solution. The loss of entropy is $nR \log_\epsilon (p_0'/p_0)$, where $p_0'/p_0 = 1 \cdot 0936$.

(f) Compress the air isothermally into solution at 20° C. through a semi-permeable membrane. The loss of entropy—compare stage (c)—is

$$\frac{nL'_{20}}{293} + nR \log_\epsilon \frac{\pi}{p_0'}.$$

Equating to zero the whole entropy-loss for the cycle we find that

$$\int_{288}^{293} \frac{S_2 - S_3}{T} dT + nR \log_\epsilon \frac{\pi}{p_1} + n \frac{L'_{20}}{293} - n \frac{L'_{15}}{288} - 7 \cdot 02\, n \log_\epsilon \frac{293}{288} = 0,$$

$$\int_{288}^{293} \frac{S_2 - S_3}{T} dT = 8 \cdot 4 \times 10^{-8} \left[-1 \cdot 986 \log_\epsilon 1 \cdot 094 - \frac{2230}{293} + \frac{2570}{288} + 7 \cdot 02 \log_\epsilon \frac{293}{288} \right]$$

$$= 1 \cdot 04 \times 10^{-7}.$$

Assuming as a sufficient approximation that $(S_2 - S_3)$ does not vary with temperature, we find that

$$S_2 - S_3 = 1 \cdot 04 \times 10^{-7}/\log_\epsilon (293/288)$$

$$= 6 \cdot 0 \times 10^{-6}.$$

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539.164.1

THE RANGES OF α PARTICLES IN PHOTOGRAPHIC EMULSIONS

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ABSTRACT. Numerous α-particle tracks on Ilford *R* plates have been measured. From measurements of the tracks of a homogeneous group of α particles of range *R*, a distribution curve may be drawn, from which a length *L*, known as the extrapolated length of the tracks, can be determined. The ratio *L/R* is shown to be constant to within 1 per cent for α particles of various ranges, and hence *R* can be found for a group of unknown tracks if *L* is measured. The method is applied to determine the ranges corresponding to the tracks produced by the disintegration of boron and lithium under slow-neutron bombardment. These are found to be 1.14 ± 0.02 cm. in air and 6.64 ± 0.06 cm. in air, respectively. Samarium has also been investigated, and is found to emit α particles of range 1.13 ± 0.02 cm. in air. Other particles from samarium, of longer range, have also been found.

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§ 1. INTRODUCTION

IT has been known for many years that it is possible to record the tracks of α particles in a photographic emulsion. In earlier papers [1,2] estimates are given of the mean length of such tracks for α particles of known energy, and these accord quite well with recent measurements. The photographic method of recording heavy particles has recently been shown to have important applications, and the technique has been improved. It has therefore become necessary to make a closer study of the relation between the lengths of the tracks measured in the emulsion and the corresponding ranges in air. The purpose of the present paper is to discuss this relation and to decide the degree of accuracy with which it is possible to determine the range in air of a group of α particles, on the basis of measurements of the length of the tracks produced in the emulsion.

If a homogeneous group of α particles is caused to traverse the emulsion, the tracks produced by the individual α particles are not all of the same length. The variations have been shown in a previous paper [3] to be due to the following causes: (a) straggling of the ordinary type; (b) additional straggling introduced by the inhomogeneity of the medium; (c) the difference between the actual distance traversed by the particle and the measured distance between the end grains; (d) uncertainty whether the whole path of the particle is contained within the emulsion.

The variations due to these four factors are in increasing order of magnitude. For α-particle tracks, and short tracks generally, (d) may be excluded; it becomes of importance only in the case of long tracks for which the number of grains per unit length is small, such as the proton recoil tracks produced by neutrons [3].

Reasons were given in the previous paper[3] for supposing that the combined effect of (*a*), (*b*), and (*c*) would be very appreciable. Thus for the α particles from thorium C′, having a range in air of 8·6 cm., one would expect to find differences amounting to 20 per cent between the maximum and minimum measured lengths in, say, 100 measurements. This conclusion is confirmed by the observations. In a group of 400 thorium C′ α-particle tracks recently measured, the maximum and minimum lengths were 31·0 and 22·9 scale divisions, the extent of variation being thus 26 per cent of the maximum. Smaller groups show a variation of about 20 per cent.

It is to be observed that the discrepancy (*c*) represents the amount by which the end grains fall short of being exactly at the ends of the true path, and this amount is independent of the length of the track. Thus the actual magnitude of the error introduced will be approximately the same for particles of all ranges, so that the relative difference between the maximum and minimum measured lengths will be greater, the smaller the range of the particles. This conclusion also is borne out by the measurements. In 200 measurements of the tracks produced by thorium C α particles, having a range in air of 4·7 cm., the minimum shows a variation of 35 per cent with respect to the maximum. In 300 measurements of the short-range α particles of samarium, having a range in air of 1·13 cm., the maximum and minimum measured lengths are 4·0 and 1·2 scale divisions.

In determining, therefore, the range which a particle would have in air, by measuring its track in the emulsion, large errors are unavoidable. It was for this reason that the method was found unsuitable for determining the detailed distribution of neutron energies from measurements of the tracks of recoil protons[3]. Where, however, it is possible to measure a large number of tracks produced by particles all of the same energy, much higher accuracy is attainable in determining the corresponding range in air. We have examined this question on the basis of large numbers of measurements of the tracks produced by α particles of known energy.

§ 2. DEFINITION OF *L*

L

R

The problem before us is twofold. Firstly, we have to determine from the measurements a single representative value L which will define the length of the tracks. We have secondly to find the relation between L and the range R in air. The selection of the best representative value is by no means easy. Even in air the individual ranges of α particles of the same energy show small variations due to straggling, and the value to be taken as the range in air has to be defined. Thus we have the extrapolated range, derived from the Bragg ionization curve, which differs from the mean range. Either of these representative values may be taken as a measure of the range in air, and one could suggest others, such as the most probable range.

For the tracks in the emulsion several such choices are possible. The most obvious suggestion is to define L as the mean of a large number of measurements. The mean of a set of observations, however, is only a satisfactory statistic when the

conditions are satisfied (*a*) that the observations constitute a true random sample of all possible observations, and (*b*) that the distribution curve of the observations is symmetrical. In the present case neither condition is satisfied. The measurements cannot form a truly random sample, for one is obliged to select tracks for measurement which are horizontal or nearly so, and of which the end points are well-defined. Tracks which do not satisfy these criteria are rejected. There is reason to think that such a selected sample approximates to a random sample, but it is evidently not wholly so. The selective character of the measurements is apparent in the differences which appear between similar groups of tracks measured by different observers. The position of the maximum of the distribution curve may be slightly different, and one observer's measurements may show a preponderance of shorter tracks as compared with another's.

If the true paths of the particles in the emulsion could be measured one would presumably obtain a distribution curve approximately symmetrical about the mean
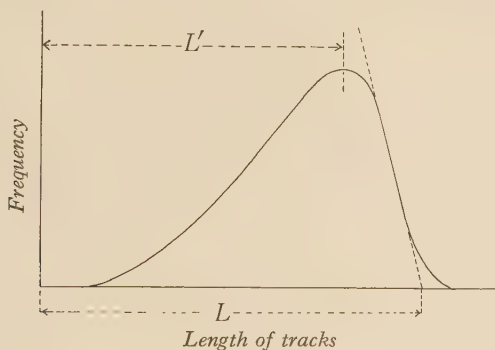
Figure 1. General form of distribution curve.

length. The actual measured distance, however, is the distance between the end grains, which do not as a rule lie at the extreme points of the path. It follows that the number of measurements which fall short of the (unknown) mean will be greatly in excess of those which exceed it. The point was considered in the previous discussion [3], and it was pointed out that the probability of a given defect of the measured length with respect to the true path is not a symmetrical function. In general, it appears that if the frequency-distribution of the measured lengths be plotted, it will have the general form shown in figure 1, and the results of the actual measurements confirm this.

The next suggestion, therefore, is to choose the value L' which corresponds to the peak of the distribution curve; and this amounts to a choice of the mode or most probable length. For reasons already stated, L' will differ from the true mean path p by an approximately constant amount, irrespective of the length of the path. Thus the ratio L'/p will decrease with p. The value of p is of course unknown, but we may suppose it to be proportional to R, the range in air. Thus the ratio L'/R will decrease with R. This is seen from table 1, where the ratio has been calculated for known α particles of different ranges.

Table I

Source of α particles	Thorium C′	Radium C′	Thorium C	Uranium II
Extrapolated range in air (cm.)	8·623	6·945	4·746	3·28
L′	61·24	48·18	32·81	21·58
L′/R	7·014	6·937	6·913	6·580

A further objection to the use of L' is that the maximum of the curve is not a point which can be determined with great accuracy. Occasionally, owing to the inevitable fluctuations in a limited group of measurements, the curve shows two peaks. Even where the peak is well-defined, its position as determined by different observers may vary slightly.

We have therefore come to the conclusion that the best representative value is that obtained by extrapolating the curve, and determining the point of intersection on the x axis. If the curves actually obtained were smooth no difficulty would arise, and the extrapolation would be made as shown in figure 1. Actually, however, a limited group of measurements gives an irregular curve, and the descending portion generally shows subsidiary maxima and minima. These irregularities are so large that extrapolation by eye estimation is impracticable, and if it were attempted the value of L would depend largely upon individual judgment. It is necessary to adopt some standard procedure in order to avoid these personal differences. The device we have adopted is to replace the descending portion of the curve by a straight line through the plotted points, computing its position by the principle of least squares. Thus if x_1, y_1; x_2, y_2; ... are the coordinates of the n points, and $y = a + bx$ is the straight line, the values of a and b are those for which $(y_r - y)^2$ is a minimum. These values are calculated from the formulae

$$a = (\Sigma x \Sigma xy - \Sigma y \Sigma x^2)/\{(\Sigma x)^2 - n . \Sigma x^2\},$$
$$b = (\Sigma x \Sigma y \quad - n . \Sigma xy)/\{(\Sigma x)^2 - n . \Sigma x^2\}.$$

We then have for the point of intersection on the x axis, $L = -a/b$.

This procedure is admittedly empirical, but it gives a value for L, the length of the tracks, which is definite, unambiguous, and free from personal errors of estimation. Further, the determination of L from a group of measurements is not unduly laborious, and it is found in practice that L has a much more nearly constant value than L'. The values of L deduced from different groups of measurements are remarkably constant, although the shape of the curves varies considerably.

§3. DETERMINATION OF L

The measurements of the lengths of the tracks show two characteristic types of error. Firstly, the plates are never wholly free from background grains which do not belong to the tracks at all. If a background grain happens to lie just beyond the end of a track it may be mistaken for an end grain, and the measured length will be greater than it should be. Nearly all our groups of measurements show one or two

tracks of excessive length which we believe to be due to this cause, and these have accordingly been ignored in the determination of L. These long tracks sometimes appear on the distribution curve as a small peak well to the right of the main curve. This can be seen in figure 2.

Another type of error in the measurements is of interest. The grains are of very small diameter, of the order of 0.3μ, and therefore what is seen in the microscope is scarcely a geometrical image of a grain, but rather a small diffraction circle. A scale division corresponds approximately to 2μ, and the readings involve the estimation of the position of the centres of the diffraction circles which correspond to the end grains. When the diffraction circle is near a scale division there is an apparent coalescence which results in an excess of measurements which are read as an integral number of scale divisions. In addition, there are personal idiosyncrasies in the estimation of tenths which cannot be wholly eliminated; these have been discussed
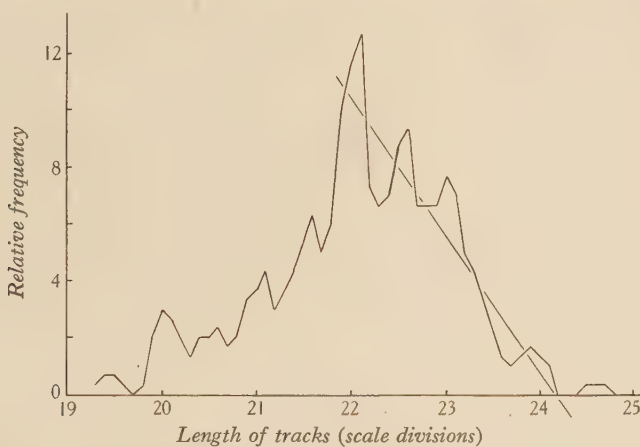
Figure 2. Distribution curve of tracks produced by radium C' α particles.

at some length by Yule[4]. We find, therefore, that the detailed distribution of the measurements is somewhat uneven, and if the frequency of occurrence is plotted directly against the measured length in scale divisions, peaks tend to occur at the whole numbers, and to a lesser extent at the mid points of the divisions. Although we have been conscious of the existence of these tendencies, and have therefore been on our guard against the careless estimation of tenths, we find that they are not wholly eliminated with increasing experience. The conclusion to be drawn is that the limit of accuracy in the measurement of the lengths of individual tracks has been reached, at least with the present optical equipment.* We estimate the probable error of an individual measurement to be of the order of $1/5$ of a scale division, or 0.4μ.

We have therefore thought it proper to smooth out minor irregularities in the curves by the device of a moving average. The direct curve would be given by

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* A dry objective of the highest available nominal aperture has been used throughout, for when large areas of the plate have to be traversed the use of an immersion objective is impracticable, and in any case it is undesirable to have oil on the gelatine surface.

plotting the number of tracks against the measured length. The smoothed curve is obtained by plotting the mean of three consecutive ordinates against the abscissa of the central ordinate. This is a well-known statistical device which has the effect of reducing minor irregularities while accentuating the general trend of the curve. It has been adopted for all the curves—indeed an even wider moving average would be justifiable, say one of five consecutive ordinates.

Before plotting the frequency curves it is necessary to correct the measurements for the inclination of the tracks. If a track is not in the horizontal plane, the vertical distance between the two ends is measured by means of the vertical motion of the microscope. This measurement can be made to 1μ. Owing to the refraction of light in the gelatine, the true vertical separation is greater than the measured distance. If the objective used were of small nominal aperture, so that the rays were all approximately parallel to the axis, it would suffice merely to multiply the readings by a factor equal to n, the refractive index of the gelatine. With a large-aperture objective, however, the image is produced by rays of which some make large angles with the axis, and the apparent depth of a grain is accordingly reduced. The objective is corrected for use with uncovered objects, so that when a grain at some depth in the emulsion is under observation the objective is, in effect, over-corrected for spherical aberration.

n

When the grain is only at a small depth in the emulsion, the over-correction is insufficient to destroy the definition of the image, but the apparent depth of the grain is less than that which would be observed with a small-aperture objective. The factor by which the observed vertical depth must be multiplied in order to obtain the true depth is greater than n, and can be approximately calculated by considering the path of a mean ray between the axial and marginal rays. It may also be estimated by measuring the thickness of the emulsion by independent methods. With the present objective the factor is approximately 2, and we have used the value 2·0 in the present measurements. The error in the measurement of depth is relatively much greater, however, than that in the measurement of length, so that only those tracks have been included for which the correction on account of inclination is small. In order to apply the correction quickly and accurately, a graphical method has been used.

§4. THE DEPENDENCE OF L UPON VARIOUS FACTORS

It is necessary to know whether the length L corresponding to a given group of particles, defined and determined as described, is a constant quantity. Its value might conceivably depend upon (*a*) the plate used, and the mode of development; (*b*) the observer; (*c*) the number of measurements included in the group. We now consider these separately.

(*a*) *The plate*. Ilford R plates, originally designed for recording these tracks, have been used throughout. The properties of these plates are remarkably constant. Measurements have been made upon plates of the early batches (August 1934), and on successive batches of plates up to the present time, and no significant differences can be detected. In particular the value of L for a given group of particles has not

varied. Moreover, the value of L is practically unaffected by changing the time of development, within wide limits. A standard metol-hydroquinone developer is used, and development is allowed to take place slowly to secure uniformity. The usual time of development is 18 min. at 18° C. Three plates were exposed in exactly the same way to a source of thorium $(B+C)$, the α particles being allowed to fall almost tangentially on the plates in a vacuum. These plates were developed for 13, 18, and 23 min. The plates show considerable difference in appearance, for the number of background grains increases with the time of development. Also the apparent density of the tracks increases with the time of development. In an undeveloped plate some of the affected grains are only partially reduced to silver by the action of the developer, and hence are less easily seen. They may, indeed, be invisible, so that the mean number of grains per unit length is found to be less for tracks on an underdeveloped plate. A consequence of this is that an underdeveloped plate may show a preponderance of relatively shorter tracks. The end point of the distribution curve, however, as defined by L, is found to be the same for all three plates, within the limits of error. This is understandable when one observes that even an underdeveloped plate will show some tracks which approach the maximum length. Extremely underdeveloped plates, however, will of course not give satisfactory results, for then the variations between individual measurements become very large.

(b) *The observer.* The measurements on which the present paper is based have been made by two observers, but the measurements have been kept separate throughout. We can find no systematic differences in the value of L between the two sets of observations. This point having been established, both sets have been combined in order to obtain the final results.

(c) *The number of measurements.* The value of L will obviously depend on the number of measurements, for in a small group the random departures from the ideal distribution are relatively more important. The largest groups number some 400 measurements, whilst several curves have been drawn with groups numbering upwards of 100. The smoothness of the curves increases with the number of measurements, but to measure very large numbers of tracks is in many cases not feasible. We have, for example, the tracks produced by the transformation of lithium under slow neutron bombardment, § 8. The best plate showing these tracks had an exposure about as long as would normally be practicable. The source of neutrons consisted of beryllium and radon of initial strength 150 millicuries, and the exposure lasted four days. Even on this plate, however, the tracks are very sparse, and up to the present we have only been able to accumulate 117 satisfactory measurements. It is unlikely that if the method were applied to other nuclear reactions the tracks obtained would usually be more numerous than these. Since the main interest of the method lies in its application to the determination of the ranges corresponding to unknown tracks of this kind, we have limited the observations to groups of not more than a few hundred measurements.

The minimum number of measurements required to give a reproducible value of L may be taken as 100. Smaller groups have not given satisfactory results.

§ 5. MIXED GROUPS OF PARTICLES

Uranium emits two groups of α particles, corresponding to the isotopes uranium I and uranium II. The ranges of these particles in air are 2·7 and 3·3 cm. It is not possible to determine by inspection whether any given track belongs to the shorter or the longer group, so that the measurements include both kinds of track in approximately equal numbers. In the case of thorium (B+C) the ranges 4·7 and 8·6 cm. of the two groups of α particles emitted differ so widely that any track can be assigned immediately to its proper group, so that the difficulty does not arise.
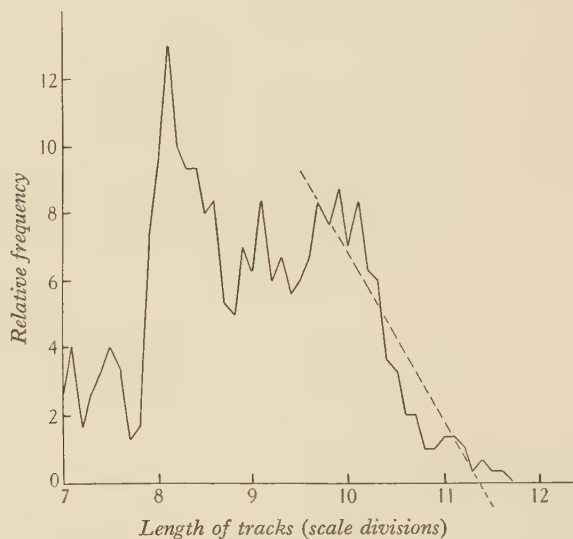
Figure 3. Distribution curve of tracks produced by uranium α particles.

The curve for uranium is shown in figure 3. If one were unaware of the origin of the tracks the existence of two groups could be argued from the curve, but not with any degree of certainty. The value of L computed in the usual way clearly applies to the longer-range group, and that corresponding to the shorter-range group cannot be determined except as a rough estimate. The example of uranium makes it clear that the present method is not adapted for separating groups of particles of closely similar ranges. We consider that two groups of particles should differ in range by at least 1 cm. of air in order to be clearly and unambiguously resolved by the present method.

§ 6. RELATIONSHIP BETWEEN L AND R

The extrapolated length L for any group of α-particle tracks is found to be proportional to R, the extrapolated range in standard air. α particles of four ranges have been used, and the results are collected in table 2. We have taken the values of R from the most recent determinations[5]. The table contains 17 determinations of L/R. These determinations refer to plates which have received very different treatment; some are extremely clean and others show a good deal of background.

The ranges of the α particles vary between 3·3 and 8·6 cm. in air, and the number of measurements in the individual groups varies between 90 and 420. It is satisfactory to find, therefore, that L/R is constant to what must be regarded as a fair degree of accuracy. The maximum and minimum values of L/R are 7·662 and 7·397, so that the deviation from the mean is never more than 2 per cent. In order to find

Table 2

Source of α particles	Series number	Number of tracks	R	L	L/R	Notes
Thorium C′	1	130	8·623	65·05	7·543	Plate I, developed in 13 min.; measurements by T.
Thorium C′	2	90	8·623	65·42	7·587	Plate I, developed in 13 min.; measurements by D.
Thorium C′	3	100	8·623	65·94	7·647	⎫ Plate II, developed in 18 min.;
Thorium C′	3	100	8·623	64·70	7·504	⎭ curve showing two maxima.
Thorium C′	4	100	8·623	64·51	7·481	Plate III, developed in 23 min.
Thorium C′	5	420	8·623	64·83	7·519	Total of all measurements.
Radium C′	6	110	6·945	52·71	7·590	Measurements by T.
Radium C′	7	90	6·945	52·10	7·502	Measurements by D.
Radium C′	8	200	6·945	52·71	7·590	Total of all measurements.
Thorium C	9	90	4·746	36·06	7·597	Plate I, developed in 13 min.; measurements by T.
Thorium C	10	119	4·746	36·36	7·662	Plate II, developed in 18 min.; measurements by D.
Thorium C	11	100	4·746	36·06	7·597	Plate II, developed in 18 min.
Thorium C	12	99	4·746	35·32	7·441	Plate III, developed in 23 min.
Thorium C	13	408	4·746	36·01	7·588	Total of all measurements.
Uranium II	14	58	3·28	—	—	Plate I, not a satisfactory curve; only a few tracks were obtainable on this plate.
Uranium II	15	179	3·28	24·26	7·397	Plate II.
Uranium II	16	237	3·28	24·76	7·550	Total of all measurements.
Uranium II	16	237	3·28	24·28	7·404	Total with alternative extrapolation.

a suitable mean, we have taken the four groups of which each contains the total measurements for each kind of track. By plotting L against R, points are obtained which lie very closely on a straight line. The best straight line amongst these points is computed by the method of least squares, and its slope gives what is probably the best value of the ratio. In view of the slight ambiguity in the value of L/R for the uranium tracks, equal weight has been given to both values, which are the last two values in table 2.

The mean value of L/R so determined is found to be 7·543. If x represents the probable error in this value, about half the determinations ought to have deviations greater than x, and about one-sixth ought to have deviations greater than $2x$. Out of the 17 values in the table, 12 lie well within 1 per cent of the mean, and no deviation exceeds 2 per cent. It follows that probably x is appreciably less than 1 per cent of the mean value.

If x is deduced in the usual way from the deviations, the value found is 0·053. One must also take into account the fact that 17 is rather a small number of determinations upon which to base an estimate of x. Statistical theory shows that the

probable error in x will be $[0.51/\sqrt{(17)}].x$, or 0.007. If we assume the limit of error in x to be three times the probable error, we get 0.074 as the maximum possible estimate of x, which is almost exactly 1 per cent of the mean. We have therefore adopted $\pm$ 1 per cent as a safe estimate of the probable error in a single determination of L/R.

It is now possible to determine the range in air corresponding to any unknown group of tracks by applying the relationship $L/R = 7.543 \pm 0.075$. We have therefore used this to determine (a) more accurate values of the ranges corresponding to the tracks which result from the disintegration of boron and lithium by slow neutrons; and (b) the range of the α particles of samarium.

§ 7. BORON-DISINTEGRATION TRACKS

It has been shown previously [6, 7] that boron, when bombarded by slow neutrons, disintegrates according to the following scheme:

$$B^{10} + n^1 \rightarrow Li^7 + He^4.$$

Tracks have been obtained which correspond in length to the total range of the two resultant particles. The previous best estimate of this total range was 1.1 ± 0.1 cm. in air, which agrees with the measurements of Kurtchatow, Kurtchatow and Latychew in a Wilson chamber [8].

We have now computed the extrapolated length of the tracks from a group of 329 measurements made on the best plate. Only horizontal tracks have been measured, to avoid errors due to inclination. The value of the range in air is found to be 1.14 cm. Owing to the shortness of the tracks the accuracy with which L can be determined for these tracks is doubtless somewhat less than for the α-particle tracks already considered. Also, though L/R has been shown to be constant for α particles of range between 3.3 and 8.6 cm., it is perhaps unsafe to extrapolate this result to extremely small ranges. To take these considerations into account the value assigned to the probable error must be increased. We take the following value to be the best at present:

$$R = 1.14 \pm 0.02 \text{ cm.}$$

E We have now to determine, if possible, the energy associated with this range. The total energy E is divided between the particles in the inverse ratio of their masses, so that the velocities of the particles are known in terms of E. If then the {range, velocity} relation is known for each kind of particle, the total range corresponding to E can be found. Conversely, one can find by trial the value of E corresponding to the given range.

Unfortunately the {range, velocity} relation for lithium recoil atoms is unknown, and the uncertainty arising from this cause is greater than that involved in the measurement of R. One may, however, make use of the formula

$$R = km\,z^{-n}f(v) \qquad\qquad \dots\dots(1),$$

where m, z, and v represent the mass, nuclear charge, and velocity of the particle, and k is a constant. For fast protons and α particles we may put $n = 2$, for an

α particle has approximately the same range as a proton of the same velocity. For much heavier recoil atoms, N^{14}, O^{16}, O^{17}, A^{40}, F^{19}, the formula has been shown to represent the observations approximately, with n equal to $\frac{1}{2}$ [9, 10]. The best one can do, therefore, in the absence of observations on lithium recoil atoms, is to use the formula and assume n to be somewhat less than 2. Calculation with various values of n gives an idea of the uncertainty involved.

This procedure leads to the following estimate for the total energy released in the reaction

$$E = 2 \cdot 25 \pm 0 \cdot 10 \times 10^6 \text{ eV}.$$

This value agrees, within the limits of error, with that which may be deduced from our present knowledge [11, 12, 13] of the masses of the particles involved in the reaction.

§ 8. LITHIUM-DISINTEGRATION TRACKS

It has already been reported [7] that tracks are obtained which correspond to the reaction

$$Li^6 + n^1 \rightarrow He^4 + H^3.$$
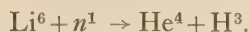
An example is shown in figure 4. The previous estimate of the total range for these tracks, $6 \cdot 9 \pm 0 \cdot 2$ cm. in air, depended upon the position of the maximum of the distribution curve, and only a small group of measurements was available. For
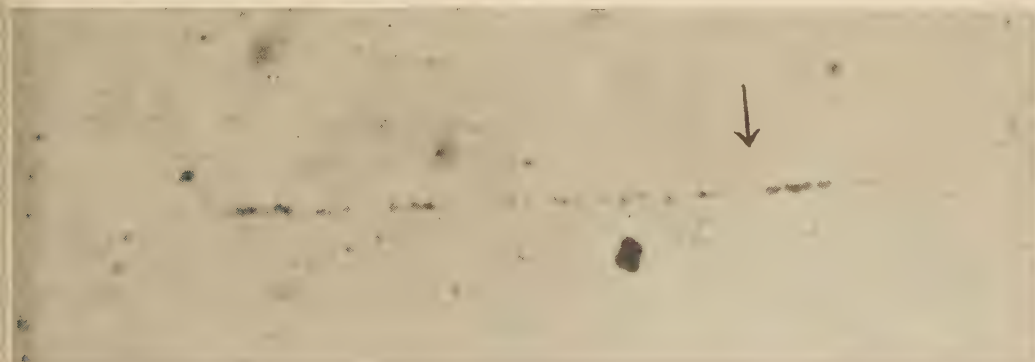
Figure 4. Disintegration of lithium by slow neutrons. The arrow shows the point of disintegration.

the present determination we have limited ourselves to tracks for which the inclination to the horizontal is small, and whose end points are clear and unambiguous. The tracks are infrequent, and the number of satisfactory measurements so far obtained is 117. These are found to give the distribution curve shown in figure 5, and the value of the range in standard air deduced in the usual way is

$$R = 6 \cdot 64 \pm 0 \cdot 06 \text{ cm}.$$

In this result the probable error given is that which is inherent in the determination, according to the considerations already outlined. There is another source of error of unknown, though probably small, magnitude, namely the fact that we do not know whether the ratio L/R is the same for hydrogen nuclei as for α particles.

In other words, it is not known whether the stopping-power of the emulsion relative to air is the same for the two kinds of particle, and there is no means of determining this in the absence of sources giving homogeneous groups of hydrogen nuclei. Experiments have been made with the protons ejected from paraffin by polonium α particles, which lead to the view that the difference of stopping-power is not large. In any case, there are no accurate data available on the {range, energy} relation of H^3 particles of the range occurring in this reaction, so that further discussion of this source of error would be premature.
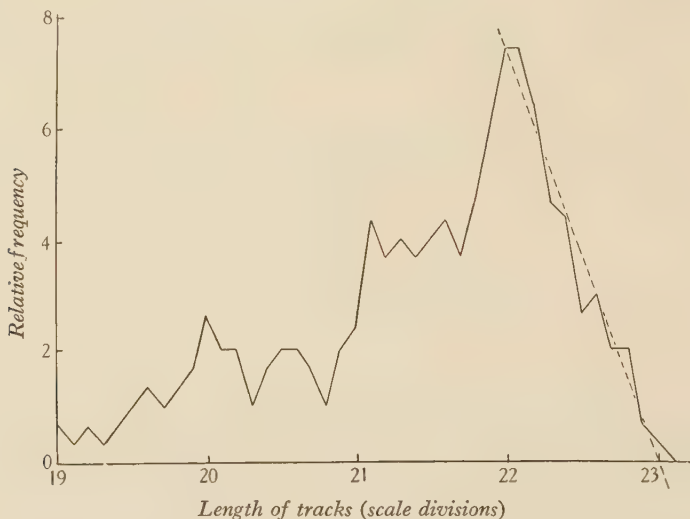
Figure 5. Distribution curve of the lithium-disintegration tracks.

To calculate the total energy released in the reaction, we have assumed the validity of formula (1), and deduced the {range, energy} relation from that which is known for protons. The value so derived is

$$E = 4\cdot3 \pm 0\cdot1 \times 10^6 \text{ eV}.$$

If we assume the recently proposed values for the masses of neutral atoms we have [11]

$$n^1 = 1\cdot0083 \pm 0\cdot0003, \qquad H^3 = 3\cdot0161 \pm 0\cdot0003,$$
$$Li^6 = 6\cdot0163 \pm 0\cdot0006, \qquad He^4 = 4\cdot0034 \pm 0\cdot0004.$$

The balance of energy in the reaction is thus seen to be $0\cdot0051 \pm 0\cdot0008$ mass units, or $4\cdot7 \pm 0\cdot8 \times 10^6$ eV., which is in good agreement with the value derived above.*

§9. THE PARTICLES FROM SAMARIUM

Several papers have been published [15,16,17,18] on the radioactivity of samarium. The general result is that samarium emits α particles of range about $1\cdot1$ cm. and

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* Budnizki, Kurtschatow, and Latychew [14] give the ranges of the He^4 and H^3 particles as $2\cdot0$ and $6\cdot5$ cm. in air, as measured in a Wilson chamber. If momentum is conserved, the ratio of the ranges should be much greater. Furthermore the total energy, as deduced from the range of the α particle, is $7\cdot9 \times 10^6$ eV., which is clearly too large.

possibly particles of longer range. Mäder, indeed, definitely reports the existence of a group of protons of range 1·37 cm., but so far this result has not been confirmed by other observers.

The tracks of the particles have therefore been obtained on a photographic plate, by soaking the plate in a solution of samarium sulphate. Since the solubility is low the concentration is small, and long exposures, of the order of a month, are required in order to obtain a large number of tracks. The plate on which the measurements have been made shows about 25,000 tracks per cm.², but for reasons already given, very few of these are suitable for measurement. For increased accuracy, we have restricted ourselves to tracks which are almost exactly horizontal. The number of measurements in the group is 310.
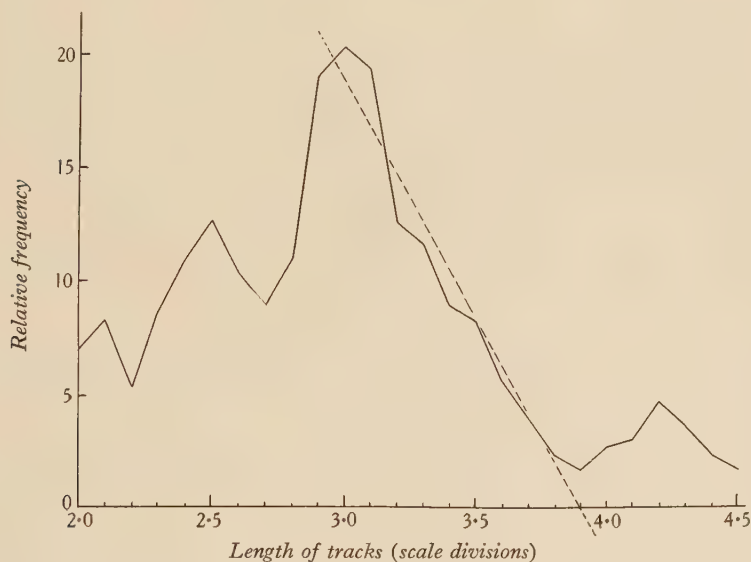
Figure 6. Distribution curve of the tracks produced by the α particles of samarium. The peak on the extreme right represents the beginning of the distribution curve of the longer tracks.

The distribution curve is shown in figure 6. It is apparent that by far the great majority of the tracks can be ascribed to the short-range α particles. We can determine the extrapolated length in the usual way, and calculate the range in air. The probable error in the determination will be the same as for the boron-disintegration tracks, since the number of observations and the actual range are approximately the same for both. We find the value

$$R = 1·13 \pm 0·02 \text{ cm.}$$

This value is in agreement with previous determinations, but is appreciably more accurate.

It is evident from the measurements that there are particles present of much greater range. The measurements are not inconsistent with the existence of the group of protons reported by Mäder, but do not definitely confirm it. It is manifest, however, that particles of range up to about 3·5 cm. in air are present, and for reasons

already outlined [19] we are strongly of the view that these are singly-charged particles and not α particles. We hope to give a fuller account of these long-range particles at a later date.

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532.13:546.681

THE DETERMINATION OF THE VISCOSITY OF LIQUID GALLIUM OVER AN EXTENDED RANGE OF TEMPERATURE

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ABSTRACT. On theoretical grounds measurements of the viscosities of liquids over as wide a temperature-range as possible are desired. Gallium is particularly suited for this purpose, and the viscosity of the molten metal has been measured between its melting point (approximately 30° C.) and 1100° C. This is a much wider range of temperature than has hitherto been recorded in viscosity-measurements. It has been necessary to devise a special technique on account of practical difficulties connected with surface films. At 1100° C. the viscosity decreases by only 2 per cent per 100°.

§ 1. INTRODUCTION

RECENT theoretical work on the subject of liquid viscosity by Prof. E. N. da C. Andrade[1] has made it specially desirable that the viscosity data for elementary substances should be augmented. In this connexion the liquid metals form an important group, but, except in the case of mercury, reliable viscosity-measurements have been made in a few cases only, and even in these cases the temperature-range is comparatively restricted. The work described here was undertaken with the object of investigating the viscosity of an elementary substance over as wide a range of temperature as possible. Gallium was chosen for these experiments on account of its low melting-point (30° C.) and high boiling point (unknown, but certainly above 1600° C.), which make it exceptionally suitable for the purposes of the present research. Actually, with the technique developed, it has been found possible to measure the viscosity of liquid gallium between its melting-point and 1100° C.

The first question which arose was that of choosing a method suitable for viscosity-determinations with a substance of which, owing to its high cost, only 2 or 3 cm³ were available. Of the variety of methods which have been developed for measuring viscosities, only the capillary-tube method seemed feasible with such quantities. This method was, therefore, adopted, in spite of unexpected difficulties encountered in the manipulation of the liquid gallium as a result of the existence of an obstinate surface film. To overcome this film a special technique was worked out, an account of which is given below.

Before proceeding to the description of the experimental work, it may be mentioned that the validity of the assumption that there is no slip at a solid boundary

during laminar flow has been questioned from time to time in the case of mercury. This question has an important bearing on the present experiment, since the behaviour of gallium, after the treatment described in this paper, is very similar to that of mercury. In a critical summary of the situation, however, S. Erk[2] has come to the conclusion that there is no valid evidence for the existence of slip in the case of mercury, the arguments in favour of its existence being based upon experiments where other effects, such as turbulence, or corrosion of the material of the capillary tube, have produced disturbing results.

The gallium used in this investigation was not spectroscopically pure, but while minute quantities of impurities have a marked effect on such properties as surface tension and on the mechanical properties of a metal in the solid state, their effect on liquid viscosity is relatively very small. The impurities were estimated not to exceed 0·1 per cent.

§ 2. PRELIMINARY EXPERIMENTS

In the preliminary experiments it was found that liquid gallium wetted both glass and quartz when manipulated in the presence of the atmosphere. Thus, when placed in a viscometer, it presented no definite meniscus on which observations could be made, and it stuck fast in the narrow capillary tube. Immediately it became evident that the simple procedure normally followed in determinations with the Ostwald viscometer would have to be modified considerably. Before any viscosity measurements could be made, therefore, the gallium had to be obtained in a state in which the essential conditions for the determination of liquid viscosities by the capillary-tube method were satisfied, namely (i) that the liquid shall flow freely without interruption under the smallest pressure-difference, and (ii) that it shall present a definite meniscus on which measurements of the rate of flow can be made.

It was found that gallium no longer adhered to the glass when there was a little dilute hydrochloric acid present. The sticking first observed, therefore, was thought to be due to a surface film of oxide. Although the action of the dilute acid on the gallium was very slow, it was quite sufficient to counteract the effect of the atmosphere, and the metal in contact with the acid behaved like a clean globule of mercury.

The next step was to get the gallium to behave similarly without the presence of the acid. This was done by placing some gallium, together with a little dilute hydrochloric acid, in a glass container and evacuating through suitable traps. The gallium showed no tendency to adhere to the glass, even after the last trace of acid had been eliminated. Thus it was obtained *in vacuo* in what will henceforth be referred to as the *flowing state*. As soon as air was readmitted, however, the gallium began to adhere to the walls of its container, as it had done in the first instance.

It was now desirable to find, if possible, some gas having no effect upon gallium, since the usual method of resetting a capillary viscometer is to restore the differences of head by means of gas pressure. With this object in view various gases, both

neutral and reducing—nitrogen, carbon dioxide, hydrogen and argon—from which traces of oxygen had been carefully removed, were tried. No gas could be found, however, in which the behaviour of the gallium was satisfactory. It was decided, therefore, to design a viscometer in which measurements could be made *in vacuo*.

As a precaution against contamination, the above tests were carried out with a few grammes separated from the main stock of gallium.

§ 3. FIRST APPARATUS

The apparatus which was made for the purpose of finding out if the vacuum method was practicable is shown in figure 1, and was made of soft glass. It is drawn approximately to scale, the actual height of the apparatus being about 27 cm. Gallium was first of all placed in the reservoir C with a little dilute acid. On evacuation the acid evaporated, leaving the metal in the flowing state. It was then introduced into the flow-tube part of the apparatus through the side tube to the left of C, and made to flow round to the other side through a fine capillary tube D of internal diameter 0·04 cm. The viscometer was clamped to a suitable
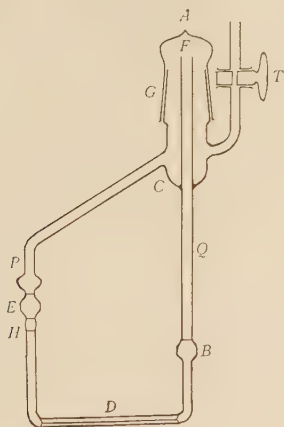
Figure 1.

holder at the points P and Q and mounted in a thermostat. The mounting of the viscometer allowed it to be tilted so that the gallium flowed into the bulb above E on the left-hand side. To take a reading, the viscometer was turned back to an upright position, and the meniscus was timed between the marks at the top and bottom respectively of the bulb E. The amount of gallium was adjusted to extend between the mark H and the mark at the top of bulb B on the right-hand side. The provision of a cover A, adapted to the apparatus by means of the ground joint G, facilitated the operations of the cleaning, filling and emptying and the extension of the tube F above the top of G helped further in this direction.

Readings were obtained between 30° C. and 50° C., and the results are shown in table 1. The apparatus was calibrated with pure benzene. It would, of course, have been better to carry out the calibrations with mercury since this liquid more

Table 1. Preliminary measurements of the viscosity of gallium

Substance	Temperature (°C.)	Time (sec.)	Density ρ	η/ρ (Stokes)	Viscosity η (poise)
Benzene	30·5	585·6	0·8675	—	0·00560
Mercury	30·5	105·3	13·52	—	0·0150
Gallium	30·5	295·0	6·095	0·003231	0·01969
	30·5	292·0	6·095	0·003200	0·01950
	35·0	288·2	6·091	0·003158	0·01924
	40·0	282·1	6·088	0·003087	0·01879
	50·0	269·9	6·080	0·002984	0·01814

nearly resembles gallium in its properties than benzene does; but the kinetic-energy correction to be applied to readings obtained with mercury was too large for that liquid to be used otherwise than for the purpose of obtaining an approximate value of a corresponding, but smaller, correction to be applied to the gallium readings. In the case of the final apparatus, however, it was found possible to use mercury for the whole of the calibration. Since the melting-point of gallium is somewhat above room-temperature, precautions had to be taken against its solidification in the viscometer. The substance could often be super-cooled considerably, but it could never be relied upon to remain in the liquid state indefinitely when it was at a temperature below the melting-point, and solidification would probably have broken the apparatus since gallium expands appreciably in the process. The density in the liquid state at the melting point is 6·095 g./cm³, while in the solid state at the same temperature the density falls to 5·904 g./cm³.[3]

The experience gained with this preliminary apparatus was sufficient to prove the feasibility of the vacuum method, and it was decided to employ similar principles in the design of a quartz viscometer for use in an electric furnace at high temperatures. Before designing such an apparatus it was necessary to know something of the probable behaviour of gallium at high temperatures, and particularly of the effect of the chloride which must have been present after the metal had undergone the treatment described above.

A paper by Sylvester Boyer[4] on the use of gallium in a high-temperature thermometer gives valuable information on the manipulation of gallium at high temperatures, and confirms one or two observations made by the writer before he was aware of Boyer's work.

Boyer, like the writer, experienced trouble due to the wetting of quartz and glass by gallium, and attributed it to surface oxidation. In the technique he evolved to overcome this, Boyer also made use of the reaction between the surface oxides of gallium and dilute hydrochloric acid to prevent oxidation during the evacuation of his apparatus. When the gallium was heated the chloride was found to volatilize, leaving the metal brighter than before and showing no signs of wetting its container. Boyer alludes to two chlorides with boiling-points one at about 210° C. and the other at 535° C.

This volatilization of the chlorides on heating was verified by the author of the present paper. The gallium was treated with dilute hydrochloric acid in an evacuated

quartz glass container and heated. A thin grey deposit, evidently chloride, was formed on parts of the container which were not directly heated, while the surface of the gallium became somewhat brighter. A consideration in the design of a viscometer for use with gallium at high temperatures was, therefore, the prevention of contamination of important parts of the apparatus by the chloride evolved during the preliminary treatment.

§ 4. DESIGN OF AN APPARATUS FOR HIGH-TEMPERATURE MEASUREMENTS

The following considerations had to be borne in mind. (i) The gallium was to be kept *in vacuo*, and therefore, the setting of the apparatus must be by tilting. Owing to the success obtained with the preliminary apparatus, it was, in fact, desirable to follow the principles used in its design as closely as possible. (ii) For satisfactory uniformity of temperature the internal diameter of the furnace to be used for the high-temperature measurements must be fairly small in comparison with its length. Therefore the viscometer must be designed to fit inside a tube only a few inches in diameter. (iii) The apparatus must be reasonably easy to clean, fill and empty. (iv) It was desirable, for simplicity, to carry out the pretreatment of the gallium and the measurements in the same unit.

Two methods of determining the time of flow of the gallium in the viscometer were available. Either the flow could be observed directly by eye or, since gallium is a good conductor of electricity, the rate of flow could be determined by movement with respect to a system of electrical contacts in fixed positions inside the viscometer. The chief objections to the electrical contact method were as follows. (i) The difficulty of sealing a metal wire into quartz glass (which would, of course, have to be the material used for the construction of the viscometer) and of making the resultant seal vacuum-tight over an extended range of temperature. (ii) The possible contamination of the gallium and of the mercury used in the calibration by the metal used for the contact wires, and of the wires themselves by the liquids used in cleaning the apparatus; and finally (iii) the impossibility of observing whether any tailing takes place at the contacts as the gallium meniscus traverses them.

On the other hand, the method of direct observation possesses obvious advantages, both as regards accuracy and convenience. The only difficulty was to arrange suitable windows in the furnace for illumination and observation.

It was, of course, realized that the method of time-measurement employed in many investigations on liquid viscosities at high temperatures was one using electrical contacts (see e.g. F. Sauerwald[5], Sauerwald and Toepler[6], and Goodwin and Mailey[7]), but in all these cases it was possible to design the viscometer in such a way that the part carrying the contacts could be removed as a separate unit. In the present case, however, the form of the apparatus was restricted on account of gas pressure not being available for resetting, and it was, therefore, not possible to arrange for such a simplification.

The quartz apparatus described below is, so far as is known to the writer, after

examination of the literature, the only viscometer which has been designed for use at high temperatures where the flow may be observed directly, and where the presence of gas in contact with the substance under investigation is dispensed with.* It has been found quite satisfactory in use at temperatures above 1000° C., and might possibly be used, with minor alterations, for measurements with other substances.

§ 5. DESCRIPTION OF THE APPARATUS AND THE METHOD OF USING IT

The apparatus which satisfied all the conditions laid down above is drawn to scale in figure 2. It was about 47 cm. in length and was made to fit, with a small clearance, inside an electric furnace, the tube of which was about 7·5 cm. in diameter and about 42 cm. in length. The actual viscometer was contained in the rectangle *ELIN*; thus, when the viscometer was in position inside the furnace the long tube *B* projected outside, and the joints *C* and *D* and the tap *T*, at the end of *B*, remained quite cool and could be sealed with vacuum grease in the usual way. The material of the covers *F* and *G*, fitting on to *C* and *D* respectively, was, therefore, pyrex glass instead of quartz glass. *F* was only to give access to the tube *A* during cleaning, while *G* carried the tap *T* through which the viscometer was evacuated,
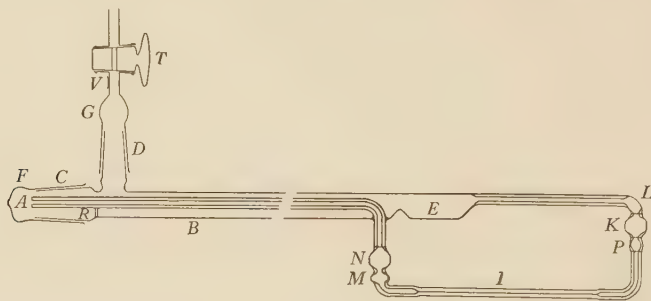
Figure 2.

and provided a more convenient opening for the introduction of the gallium. As a precaution against breakage the long inner tube *A* was supported near its end by a small pillar *R*.

To fill the viscometer, a suitable amount of gallium was placed in the small reservoir *E* and treated with hydrochloric acid in order that it might be in the flowing state *in vacuo*. This was done at room-temperature. The viscometer was then adjusted inside the furnace in the position it was to occupy when the measurements were being made, the gallium being in the reservoir *E*. The clamp supporting the viscometer held the tube *B* at a point outside the furnace and was fixed to a rail-and-slider arrangement allowing easy movement of the apparatus in and out of the furnace tube. The temperature of the furnace was allowed to rise until it

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* Recently a reference has appeared (*Science Abstracts*, January, 1935) to work carried out by S. Dobiński[8] on the viscosity of liquid phosphorus *in vacuo* with an Ostwald viscometer between the temperatures 17·5 and 80° C. A copy of the paper is not yet available to the writer.

was above 600° and it was then kept steady for about 2 hours. This treatment drove off the chloride from the gallium, together with any dissolved gas which might have been present. Pumping through the tap T was kept up all the time with a Hyvac pump. The first sign of deposition of gallium chloride was always observed on the inside of the tube B, just where it emerged from the furnace, at a place where the temperature was a little below 250° C. Chloride was deposited on the cool parts only of the apparatus, and thus the essential parts, including the bulbs and capillary tube which were inside the furnace, were not contaminated. At the end of 2 hours the furnace was allowed to cool down to about 200° C., at which temperature the filling of the viscometer could be carried out comfortably. To fill the viscometer, the gallium was allowed slowly to approach the entrance of a fine capillary I of diameter 0·04 cm. It was made to flow through I by inclining the viscometer slightly and tapping it gently with a wooden ruler. This was the most difficult operation in these experiments. Great care had to be observed in preventing the gallium from breaking up into small threads which, like those of mercury, stick fast in a fine capillary tube. Any possibility of solidification of the gallium during the filling operations was eliminated by holding the viscometer with its end projecting into the furnace tube.

After filling, the viscometer was adjusted inside the furnace tube so that the capillary I occupied a central position. The essential parts of the viscometer, occupying a length of about 17·5 cm., were thus in a position where the temperature was uniform. Actually the largest temperature-difference observed between the ends of the viscometer at high temperatures was 2° C. To adjust the viscometer in a vertical plane, it was aligned with a plumb line from the end-on position, and its inclination could be reproduced by means of a fixed optical-lever system used in conjunction with the small galvanometer mirror cemented to the apparatus at V.

The furnace tube was closed in by means of close-fitting asbestos ends. These both contained small double mica windows to allow observation of the viscometer inside. Windows were provided at both ends in order that a beam of illumination might be projected through the tube of the furnace.

To make a measurement, the furnace was adjusted to the required temperature and a cathetometer was arranged so that the bulb K of the viscometer could be observed. The viscometer was set for a reading by tilting the furnace so that the gallium meniscus rose above the mark at the top of the bulb K. The clamp holding the viscometer was, of course, connected to the outside of the furnace, so that any movement of the furnace was shared by the viscometer. In order that the tilting might be carried out conveniently the furnace, which was of the usual box type, was mounted on trunnions. As soon as sufficient gallium had flowed into the right-hand limb of the viscometer the furnace was tipped back to its former position and secured by stops attached to the trunnions, and the passage of the gallium between the marks at the top and bottom respectively of the bulb K was timed with a stop watch in the familiar way.

This process could be repeated as often as desired. Usually five readings of the time of transpiration were taken at each temperature.

It is worth mentioning that since the marks themselves were rather difficult to keep in view it was found much more convenient to adjust the cathetometer cross-wires to the marks at leisure, and then to time the meniscus past the cross wires.

A small pocket L above the bulb K was provided in order that the meniscus might not pass the mark too soon to be observable, while a margin bulb M was made below the bulb N to obviate the risk of the gallium all flowing into the right-hand side of the viscometer during setting.

Thermocouples were used for the measurement of the temperatures. An iron-constantan couple was used between 150° and 600° C., while above 600° C. a platino-rhodium couple was used. The thermo-electric e.m.fs. were measured with a Crompton potentiometer. Calibrations were made at the melting points of suitable metals, and were repeated from time to time during the course of the work. No change was detected, however. The possible error in the measurements with the iron-constantan couple was between 1° and 2° C. In the case of the platinum-platinum-rhodium couple the possible error was about 3° C., the error being larger on account of the smaller thermo-electric e.m.f. In both cases the error introduced into the viscosity measurements is negligible.

At temperatures above 800° C. mica is not a suitable material for the observation windows, and quartz glass was substituted. The joints C and D and the tap T always remained quite cool, whatever the furnace-temperature. As a special precaution against contamination of the gallium, however, apiezon grease, which is known to have an extremely low vapour-pressure, was used on the joints in place of ordinary vacuum grease during the measurements at 800° C. and above.

§ 6. CALIBRATION

The calibration of the viscometer was carried out with mercury. It was not possible to choose the dimensions of the apparatus in such a way that the kinetic-energy (sometimes referred to as the Hagenbach-Couette) correction could be neglected, so it was determined directly by experiment. This seems to be the most satisfactory method of applying the correction in view of the fact that there is still some uncertainty about its magnitude, even in the case of viscometers in which the ends of the capillary are submerged in the liquid.

η The equation* for the determination of the viscosity η of a liquid with a visco-meter of this type is

$$\eta = A'\rho T - B\frac{\rho}{T},$$

T, ρ where T is the time of transpiration of the liquid in the viscometer, ρ its density,
A', B and A' and B are constants for the instrument. The term $B\rho/T$ is the kinetic-energy correction term. Calibration consisted, therefore, in the determination of the constants A' and B, the viscosity of the gallium at any temperature being then calculable from a knowledge of its density and time of transpiration.

To determine A' and B, the time of transpiration for mercury was obtained at

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* See, for instance, Barr[9] whose notation is adopted here.

various temperatures. B could then be found from the slope of the graph of $\eta/\rho T$ against $1/T^2$, the values of η and ρ being known from the International Critical Tables. The value of A' was obtained by dividing $\eta/\rho + \beta/T$ by the corresponding value of T at each temperature, and taking the mean.

Calibrations were carried out for two positions of the viscometer since, for some of the readings with gallium, it was found better to increase the rate of passage of the meniscus past the marks by inclining the apparatus slightly. The values of B obtained at the two inclinations used are seen to be practically the same, table 2, as they should be, since B contains terms depending only on the dimensions of the

Table 2. Calibration of the quartz viscometer with mercury

Temperature (°C.)	Time (sec.) inclination 0°	Time (sec.) inclination 5°	η/ρ (Stokes)
15	110·6	67·2	0·0011655
50	100·0	61·4	0·0010370
100	89·2	55·3	0·0009060
200	78·0	49·0	0·0007700

$$\left.\begin{array}{l} A' = 1\cdot118 \times 10^{-5} \\ B = 0\cdot00800 \end{array}\right\} \text{ inclination } 0°$$

$$\left.\begin{array}{l} A' = 1\cdot915 \times 10^{-5} \\ B = 0\cdot00833 \end{array}\right\} \text{ inclination } 5°$$

viscometer. The slight difference observed is due to the fact that B, having its origin in a small correction, is not determinable with a high degree of accuracy. Nevertheless, since the maximum kinetic-energy correction applied was only about 10 per cent, and in most cases the correction was much smaller, the actual uncertainty introduced is negligible. The mutual consistency of the values obtained for the constant A' was better than 1 part in 200, but the real accuracy naturally depends upon the precision with which the viscosity of mercury is known.

During the calibration a few readings had to be taken with slightly different quantities of mercury. This was done for the purpose of applying a small correction necessitated by the fact that the quantity of gallium could not always be adjusted exactly to fill the space in the viscometer between the mark P on the right-hand side and the mark at the top of the bulb N on the left.

Since such a big range of temperature was used in these experiments, it seemed possible that there might be a correction owing to expansion of the bulbs and capillary tube of the viscometer. When calculated, however, the magnitude of this correction was found to be only 1 part in 2000.

§7. DISCUSSION OF RESULTS

The results obtained for the viscosity of liquid gallium up to 1100° C. are given in table 3 and are shown plotted in figure 3. These readings were obtained with different fillings of gallium over a period of about 3 months. At a few places results are shown repeated at nearly the same temperature (e.g. 97·7° and 102°, and 200° and 203°); they were taken with different fillings, and indicate the consistency with which the

Table 3. Viscosity of gallium

Temperature (°C.)	Time (sec.)	η/ρ (Stokes)	Density ρ	Viscosity η (poise)
[30]	—	—	—	0·02037
52·9	165·2	0·003115	6·080	0·01894
97·7	142·3	0·002668	6·042	0·01612
102	141·7	0·002656	6·041	0·01604
149	125·7	0·002341	6·005	0·01406
200	193·1*	0·002120	5·972	0·01266
203	112·5	0·002082	5·970	0·01243
301	95·5	0·001743	5·905	0·01029
402	139·6*	0·001504	5·840	0·008783
402	84·3	0·001517	5·840	0·008858
500	78·7	0·001404	5·779	0·008113
500	131·5*	0·001409	5·779	0·008141
600	126·0*	0·001345	5·720	0·007694
600	75·9	0·001347	5·720	0·007705
604	123·0*	0·001310	5·718	0·007491
806	67·1	0·001164	5·604	0·006524
1010	103·3*	0·001077	5·492	0·005915
1100	62·3	0·001062	5·445	0·005783

* Readings taken with viscometer-inclination of 0°. All the rest were taken with an inclination of 5°.
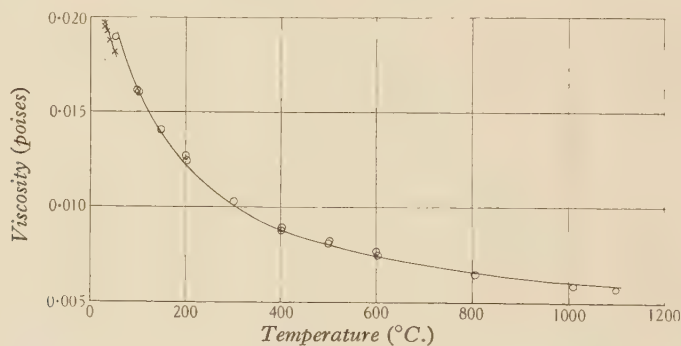
Figure 3.

results could be repeated. Pairs of results (e.g. at 500°) were taken with the same fillings to check the consistency between results obtained at the different inclinations used. The agreement is seen to be quite good.

The few results obtained with the preliminary apparatus, table 1, are shown plotted as crosses on the same graph. It will be seen that these are about 4 per cent lower than those obtained with the final quartz apparatus. This, however, is not very surprising when the differences between the two methods are remembered, as well as the fact that the calibrations were made with such different liquids as benzene and mercury. Little weight, then, is to be attached to the absolute values obtained in the preliminary experiments, but they are given here because it is worth noting that the rate of change of viscosity obtained with the first apparatus is consistent with the final measurements.

Measurements obtained with a viscometer only give the values of the kinematic viscosity η/ρ directly; therefore the value of the density of the liquid investigated must be known before the actual viscosity in poises can be calculated. The only value for the density of liquid gallium given in the literature[3] is for a temperature near its melting-point, but experiments have been made in this laboratory by W. H. Hoather to determine the density of gallium over a wide range of temperature, and my thanks are due to him for allowing me to use some of his results before publication.

The value of the viscosity of gallium at the melting-point is 0·0204 poise. This is of the same order as the value, namely 0·0146 poise, predicted from the formula

$$\eta_m = 5 \cdot 1 \times 10^{-4} \frac{(AT_m)^{\frac{1}{2}}}{V_A^{\frac{2}{3}}},$$

given by Andrade's theory, η_m being the viscosity of an elementary substance at its melting-point, T_m the absolute temperature of the melting point, A the atomic weight, and V_A the atomic volume.

Andrade's theory contemplates a close-packed cubic structure, and the agreement with experiment is close for metals possessing such a structure, while for structures which are not close-packed the value given by the formula is too small. Thus gallium, which has a complicated crystalline structure (orthorhombic with distorted close-packing) behaves as was to be expected in having a melting-point viscosity of the order of, but somewhat in excess of, that predicted.

In part II of his paper on the theory of viscosity[1] Andrade has advanced a formula to give the variation of the viscosity of a liquid with temperature. This formula is

$$\eta v^{\frac{1}{3}} = A e^{\frac{c}{vT}} \qquad \qquad \text{......(1),}$$

where v is the specific volume of the liquid at the absolute temperature T, and A and c are constants. Values of A and c have been chosen by the method of least squares, applied as described in Andrade's paper, to give the best fit between equation (1) and the experimental results. This is the largest range of temperature over which the formula has yet been fitted, and it is satisfactory to note that the agreement is reasonably good, the maximum deviation being 3·9 per cent. A comparison between the experimental values and the calculated values of the viscosity of liquid gallium at different temperatures over the whole range may be made by referring to table 4, and to figure 3, where the curve shown represents the theoretical formula with the constants found as above. It is seen from the results that the viscosity of gallium only drops to about one third of its value over a range of more than 1000° C. and, correspondingly, that the value of the constant c in equation (1) is very small compared with the values for most other liquids, excepting mercury. This slow variation of viscosity with temperature in the case of a substance with a monatomic molecule agrees well with the views put forward by Andrade.* It may be noted that at the highest temperature, 1050° C., the viscosity diminishes by only 2·3 per cent for a rise of 100° C.

* See reference (1), part II, page 724.

Table 4. Comparison with experimental results of theoretical values, calculated from equation (1). Range of η, 3·3. $A \times 10^8 = 246800$; $c = 79\cdot05$

Temperature (°C.)	η observed	η calculated	$\dfrac{\eta_{calc.} - \eta_{obs.}}{\eta_{obs.}} \times 100$
52·9	0·01894	0·01968	+3·91
100	0·01608	0·01617	+0·56
149	0·01406	0·01382	−1·71
200	0·01258	0·01215	−3·42
301	0·01029	0·01006	−2·24
402	0·008820	0·008807	−0·15
500	0·008127	0·007997	−1·60
600	0·007637	0·007409	−2·98
806	0·006524	0·006608	+1·28
1010	0·005915	0·006107	+3·34
1100	0·005783	0·005940	+2·71

The highest temperature at which measurements were made in this work has been exceeded by other investigators in determining the viscosities of molten metals, for instance by Thielmann and Wimmer[10] for iron with 2·5 per cent of carbon at 1400° C., and by Bienias and Sauerwald[11] for copper; but on account of the fairly high melting-points of most metals the ranges of temperature used have been small, not more than about 450° C. at the most. The most extensive measurements on metals, except in the case of mercury, which appear to have been made up to the present are on lead by Bienias and Sauerwald[11] between 441° and 844° C., and on tin by Pluss[12] and by Sauerwald and Toepler[6], between 280° and 750° C. So far as is known, therefore, the measurements described in this paper are the only ones which have been made on a single substance over anything like such a large range of temperature.

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DISCUSSION

Mr C. R. DARLING suggested that the author should make measurements on tin, which remains liquid over a wide range of temperature and keeps a clean surface.

THE RIPPLE METHOD OF MEASURING SURFACE TENSION*

By R. C. BROWN, B.Sc., Ph.D., Lecturer in Physics,
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ABSTRACT. An apparatus for the production, stroboscopic observation, and measurement of ripples for surface-tension determinations is described. The principal features are the use of a mirror attached to a vibrating reed as the means of obtaining intermittent illumination and a valve oscillator for actuating the dipper. Frequencies are measured by causing the oscillator to light an Osglim lamp which illuminates a stroboscopic disc attached to a phonic motor running at a constant speed.

The dynamic quality of the ripple method is discussed and it is shown that the local change of area of the surface due to the passage of ripples would be expected to be negligible.

Measurements of the fall of surface tension of a slowly adsorbed solution show that the method gives results similar to those obtained by a truly static method and it is concluded that the ripples have no retarding effect on the attainment of surface equilibrium.

§ 1. INTRODUCTION

THE equation for the propagation of surface waves under the combined influence of gravity and surface tension was put forward by Kelvin[1] as

$$v^2 = \frac{g\lambda}{2\pi} + \frac{2\pi T}{\lambda\rho},$$

v, g
λ, T, ρ

subject to certain conditions, where v is the velocity, g the acceleration due to gravity, λ the wave-length, T the surface tension, and ρ the density.

This equation provides the theoretical basis of a method of determining surface tension involving the measurement of wave-lengths and frequency. Such an experiment is rather more cumbersome than some of the commoner methods, and since the expression giving T involves the cube of the wave-length, considerable accuracy of measurement is required. It has the advantage, however, of resting on a surer theoretical foundation than some of the methods in general use, and also of being independent of contact angle.

A number of authors have worked with the ripple method and a fairly comprehensive bibliography is to be found at the end of this paper.

§ 2 DISCUSSION OF POSSIBLE TECHNIQUES

If the production and measurement of ripples are undertaken with a view to determining surface tensions, the procedures which may be adopted are as follows.

* The substance of this paper formed part of a thesis submitted for the degree of Ph.D. to the University of London.

(*a*) *Photographic.* Progressive waves are produced by a single point-, or line-source, and instantaneous photographs are taken, the time of exposure being made as short as possible. The negatives may then be measured and reductions made in order to arrive at the actual wave-lengths. Vincent[11, 12] made use of spark photography with considerable success.

(*b*) *Stationary waves.* Two synchronous sources of disturbance are used in such a way that standing waves occur on the liquid surface. Wave-lengths are then measured by means of a travelling microscope or cathetometer arranged above the tank. Authors who have described experiments involving stationary waves are Michie Smith[13], Matthiessen[2, 3], Grunmach[14], Kalähne[9], and Beckett and Sheard[15].

(*c*) *Stroboscopic.* In this method progressive waves are made to appear stationary by viewing them in intermittent light of the same frequency as the waves. It follows that the sharpness of the lines or images so observed will be improved by arranging that the duration of the flash shall be as short as possible. Methods of providing the stroboscopic illumination are numerous and may be classified as follows: (i) Light metal shutters having slits cut in them may be attached to the prongs of an electrically maintained tuning-fork. If they are so arranged that light passes through both of them when the fork is at rest, two flashes are produced for each vibration of the fork. References for this technique are: Rayleigh[4], Dorsey[6], Barnett[16], and Watson[8]. Hartridge and Peters[17] produced their ripples by means of a mechanical vibrator driven by a motor which also turned a slotted disc. This provided the interrupted illumination. Tyler[18] has used a.-c. mains to actuate both his dipper and a phonic motor carrying a slotted disc. (ii) Some authors have caused the current which drives the dipper also to operate a glow-discharge lamp of the Osglim type, a stroboscopic effect being thus obtained. Such experiments were carried out by Pfund[19] (who produced the ripples by an electrostatic method), Thatte and Nilkanthan[20], and Tyler[18]. (iii) Another procedure which suggests itself is the use of a Kerr cell in conjunction with polarized light after the manner of television experiments. (iv) A small mirror attached to a vibrating reed may be used as a source of interrupted light if the reflected beam is caused to pass through a slit at one stage of its vibration.

As far as can be ascertained methods (iii) and (iv) have not hitherto been used in connexion with ripples.

§ 3. TECHNIQUE AND APPARATUS ADOPTED

(i) *Production of waves.* In the present investigation a glass plate or style D, figure 1, was attached to the free end of a horizontal strip of iron R_1 which was clamped between metal plates at some point along its length. The natural frequency of this vibrator could then be varied by clamping at different points. Above it was held the magnet of a loud-speaker unit U_1 at such a distance as to give the required amplitude of the vibration of the reed for a given current in the energizing coils. This current was derived from a valve oscillator consisting of a single L.S. 5 valve

connected with inductances and capacities in the usual way. By a suitable choice of values for these the frequency of the a.-c. output could be varied over a wide range and adjusted finely by means of a variable condenser. For any chosen frequency the length of the vibrator was so adjusted that its natural frequency was the same as that of the output of the valve oscillator.
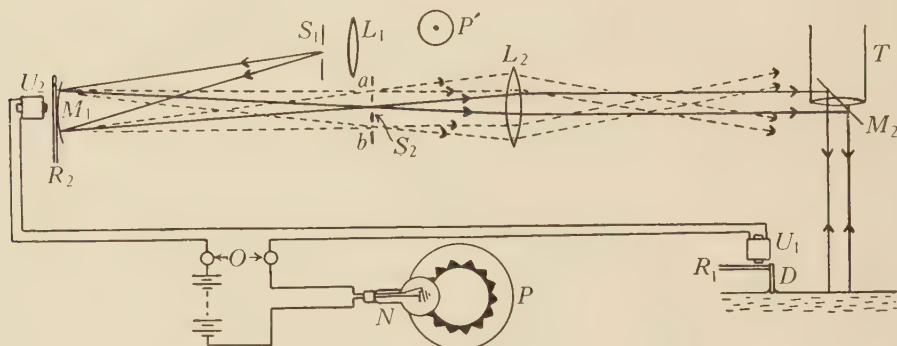
Figure 1. Diagram of ripple apparatus.

(ii) *Checking and determination of frequency.* The output of the oscillator O was connected to a small Osglim lamp N in series with a dry h.-t. battery whose voltage was adjusted so as to be just too small for the striking of the lamp. Thus the lamp gave one flash for every complete cycle of the a.-c. The lamp was made to illuminate a stroboscopic disc P mounted on the spindle of a phonic motor which was at first driven by a standard electric tuning-fork of frequency 50 and later by the a.-c. mains, which had a controlled frequency of the same value. By means of different rings containing various numbers of spots or triangles on the stroboscopic disc the oscillator could be set to produce many different frequencies, and when necessary these could be adjusted very finely so as to be perfectly reproducible. The frequency of the electric tuning-fork used initially was determined accurately by allowing it to drive the phonic wheel and noting, by means of a counter, the number of revolutions of this in a number of hours registered by a reliable chronometer. Thus, with only one standard available, waves of many different known frequencies could be used.

(iii) *Optical arrangements.* Since in previously published work the method of observing and measuring ripples with the help of intermittent light seemed to have led to greater precision than other methods this technique was adopted in the present work.

After careful consideration of all the methods enumerated in § 2(c) and after testing all except that in which the Kerr cell is used, the author decided to use the vibrating mirror. No previous record of its use in this connexion has been found. In fact, as a source of intermittent light which can very easily be set up its possibilities do not seem to have been explored at all thoroughly.

A reed R_2 similar to that carrying the dipper was clamped in an upright position, and a concave galvanometer mirror M_1 of radius 1 metre was attached to its face

near the free end. The permanent magnet and coils of a loud-speaker unit U_2 were clamped near to the reed on the opposite side to that to which the mirror was attached. The whole was mounted firmly on a levelling-table supported on three levelling-screws and provided with a vertical motion. U_1 and U_2 were connected in series with the oscillator output.

Light from a Pointolite lamp P' passed through a horizontal slit. A real image of the slit was formed at the position S_2 where another slit was placed, but the latter was afterwards found to be unnecessary. From here the light passed on through a cylindrical lens L_2, placed in such a position that S_2L_2 was equal to the focal length of L_2, and thence on to the plane mirror M_2 which was attached to the telescope T of the cathetometer. Thus the light passed from M_2 down on to the liquid surface and was then reflected nearly normally up the telescope. M_2 did not obstruct the telescope objective since it was situated alongside it, and therefore the rays incident on and reflected from the liquid surface were not quite normal to it. Adjustments of M_1, M_2 and T were made so that when M_1 was at rest a patch of light was seen in the field of the telescope. If then the a.-c. was applied to the coils behind M_1, the image formed at S_2 was spread out into a band ab, and if S_2L_2 was less than S_2M_1 the angle between the extreme rays was increased by the introduction of the lens L_2. Light reached an eye, which was looking down the telescope, twice for each vibration of M_1 and consequently, if the surface of the liquid was carrying ripples, stationary lines were seen in the field, and the distance between two alternate lines was a wave-length. The apparatus will work for any frequency provided the reeds R_1 and R_2 are made to respond to that frequency by alteration of their lengths. Apart from this and the extreme simplicity of the arrangement there is another important point in its favour. Clearly the sharpness of the lines produced by any system of stroboscopic illumination is increased if the fraction of the cycle during which the flash is made is reduced. At first L_2 was not included in the arrangement and the sharpness of the lines was improved by increasing the amplitude of the mirror M_1 and placing a narrow slit at S_2. There is a limit to the amplitude, however; moreover it was found that the slit S_2 was not effective unless it was made so narrow that the intensity of the illumination began to suffer. In other words, the limitation of the duration of illumination was being governed by the optical properties of the telescope rather than by the slit S_2. Accordingly S_2 was dispensed with and the introduction of L_2 had the desired effect.

It seems that this is the most suitable method of producing intermittent illumination of any frequency in which the fraction of the cycle during which illumination takes place is adjustable down to extremely small values. The alternative methods previously mentioned have not this advantage unless more complicated arrangements are added to them, such as inductances and condensers to modify the wave-form in the case of the Osglim lamp.

(iv) *The tank.* This was very kindly lent by Dr N. K. Adam, F.R.S., and was of the standard form used by him in surface-film experiments. After being thoroughly cleaned the inside of the tank was coated with a thin layer of wax. When in use the

tank rested on two pads of sponge rubber each 2 in. thick and placed on a concrete floor. With this arrangement vibrations were not troublesome. Small gas burners were inserted under the tank between the pads.

Waxed glass strips were used to sweep the liquid surface. The fact that these can be used to renew the surface so effectively may be recorded as one of the advantages of the method of ripples.

§4. OPERATION

On switching on the oscillator and setting its frequency at the predetermined value by means of the Osglim lamp and steadily rotating phonic wheel, lines parallel to the dipper could be seen in the telescope. These were focal lines formed by the reflection of parallel light by the concave or convex surfaces comprising the troughs and crests of the waves respectively. Thus for a given position of the telescope along the horizontal scale it was possible to obtain sharp images at two different focusings, one above and one below the liquid surface. As the line of sight of the telescope was moved away from the dipper, the two sets of images separated more and more since the amplitude and, therefore, the curvature of the waves decreased. Figure 2 is a photograph of the lines obtained when water was used, the frequency being 300 c./sec. and the wave-length 0·172 cm. An exposure of 1 minute was
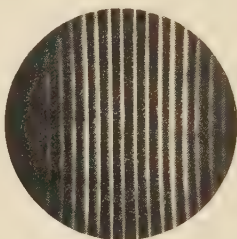
Figure 2.

necessary even with the ultra-fast plate; consequently some of the sharpness is lost owing to slight surface movements. It will be noted that maximum sharpness is shown by the central images which are produced by reflection from the troughs. In the formation of the other lines the sloping parts of the wave-outline are involved. A convenient point to work at was approximately midway between the dipper and the region where the images become indistinct. Here the telescope could be set on a series of perhaps half a dozen waves without having to be refocused, an operation which was apt to disturb its optical system and introduce errors. Alternate settings were made on the first and last lines of this restricted region and the mean of each was taken unless the readings showed that the set of waves was undergoing a general movement, as happens with a liquid whose surface tension is changing with time. In this case the difference between any particular reading and the mean of the preceding and succeeding readings gave the mean length of a known number of waves.

§ 5. THE RIPPLE METHOD AS A DYNAMIC METHOD

It was thought interesting to attempt to discover in what sense the ripple method of measuring surface tension could be called a "dynamic" method. Methods which have been described as dynamic include those of oscillating jets[21] and oscillating drops[22]. In all these the motion of the liquid surface is essential. The drop-weight, maximum bubble-pressure, and ring experiments ought to be included in the dynamic class since at the actual instant of rupture the surface is in motion, the rate of which is beyond the control of the experimenter. In the case of the method of oscillating jets, Rayleigh[23] made experiments which brought out quite clearly the dynamic nature of the method. He measured the surface tension of sodium-oleate solutions and found that the value obtained was that of pure water. Now sodium-oleate is positively adsorbed on to a water-air interface and its surface tension is that corresponding to a monomolecular layer of solute molecules provided that initially they are present in sufficient numbers in the bulk of the solution. The process of adsorption requires for completion a finite time which increases with dilution. This was measured by Bigelow and Washburn[24]. Rayleigh's results show that in the case of the oscillating-jet experiment the surface is renewed sufficiently rapidly to prevent an appreciable superficial concentration of sodium-oleate molecules from ever being established. Thus such a solution has two definite surface tensions, the truly dynamic and the truly static (such as might be determined by the method of capillary rise) and the results given by any particular method will give an indication of the degree of disturbance of the surface which the method entails. The only information of this kind which could be discovered in relation to the ripple method was that contained in a footnote on page 51 of the English translation of Freundlich's *Colloid and Capillary Chemistry*. This reads: "One might suppose that the method of surface waves leads to dynamic values. This is not the case as soon as the waves have formed properly. These new molecules of the liquid do not come continually into the waves; those remain, for the most part, which were originally present".

Consider what happens when the ripple method is used on a positively adsorbed solution. During the passage of progressive ripples over a plane surface the mean area of the surface will remain constant. There will, however, be a change in the area per small horizontal length dx, figure 3, taken along the direction of propagation. If dx is made small enough, the variation of surface area per dx is from $dx/\cos\phi$ to dx for an area of unit length perpendicular to the drawing, where ϕ is the maximum slope of the wave-outline.

Figure 3. Local change of area due to passage of ripple.

If the wave is represented by

$$y = a \sin \frac{2\pi}{\lambda}(x - vt)$$

the maximum value of dy/dx (which is $\tan\phi$) is $2\pi a/\lambda$. Both a and λ can be arrived at from measurements, and $\tan\phi$ and consequently $\cos\phi$ can be evaluated. Thus

the fractional change of area which takes place in one-quarter of the period of oscillation can be calculated.

It must be emphasized, however, that although this change of area per very small horizontal length of surface takes place at all points on the surface, yet the area per half wave-length remains constant, and therefore even if at individual points the surface concentration of dissolved molecules is changed on account of momentary contraction or expansion the average concentration over half a wave-length remains constant and will depend upon time in exactly the same way as if the surface were stationary.

During the series of readings which is recorded later the amplitude of the waves did not exceed 0·00002 cm.* and the wave-length was approximately 0·2 cm. From this we find that tan $\phi = 0·0006$ and cos $\phi = 0·9999998$, so that the maximum local variation of area would be two parts in 10,000,000 and no greater change of surface tension than this would be expected even if the mobility of the dissolved molecules were so small that the change of area resulted in a proportionate change of concentration.

There is still the possibility that some disturbance of the surface structure other than that due to local change of area might result from the passage of waves over a surface. If such an effect is present, then the value of the surface tension given by the ripple method would be that for pure water, or it might lie between this value and the final equilibrium value given by a static method. In the latter case it would be expected to depend on frequency, i.e. upon the rate of disturbance of the surface. These points were investigated.

§ 6. MEASUREMENT OF SURFACE TENSION OF SOLUTIONS OF CETYL PYRIDINIUM BROMIDE

(i) *Aqueous solutions.* At about the time when the present work on the ripple method was in progress, Dr N. K. Adam† was measuring the fall with time of the surface tension of very dilute aqueous solutions of cetyl pyridinium bromide. He used a static method, namely that involving the measurement of the dimensions of a sessile bubble, in view of the fact that this method gives values of surface tension independent of contact angle. He found that after the formation of a bubble the surface tension fell at a decreasing rate, reaching a final value only after a week or so in the case of the most dilute solutions. Thus there was to hand a liquid whose rate of attainment of surface equilibrium was exceedingly slow, and Dr Adam kindly provided as much solution as was required.

When the necessary adjustments had been made and satisfactory images were visible in the telescope, the surface was swept four times with the barriers, a stop clock being started during the last stroke. The telescope was rapidly moved and focused until lines corresponding to waves of comparatively small amplitude were

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* The method of estimating this quantity is described in the next paper.
  † Both Dr Adam's results and those presented in this paper were brought to the notice of the Faraday Society during a discussion on colloidal electrolytes; see *Trans. Faraday Society*, **31**, 204 (1935).

visible in the field. The method of observation previously described was then used, the cross wires being set alternately on two lines one wave-length apart. This procedure was continued, readings being taken every half-minute, and the frequency of the oscillator was adjusted to the exact value as often as was necessary. The first setting could be made $1\frac{1}{2}$ minutes after the fresh surface was formed. For the purpose of plotting a graph the average wave-length during successive intervals of not more than 8 minutes (and considerably less in the case of initial readings where the time change was more rapid) was calculated and recorded as the wave-length corresponding to the time centre of the interval. A graph of wave-length against time was then drawn. From this the smoothed values of the wave-length at certain convenient times were read off, the corresponding surface tensions were calculated and the final graph was thus constructed.
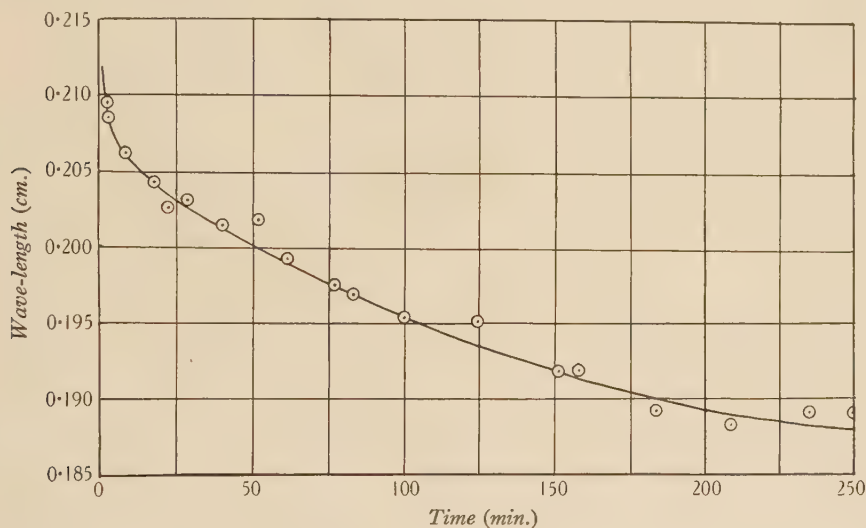
Figure 4. Change of wave-length with time. Aqueous solution of cetyl pyridinium bromide (0·003 per cent).

The curve, figure 4, shows the fall of wave-length with time for a 0·003 per cent solution at 20° C., the frequency of the ripples being 200 per second. Figure 5 is the corresponding {surface tension, time} curve. The observations shown extended over 4 hours. After $20\frac{1}{2}$ hours the same solution was found to have a surface tension of about 36 dyne/cm. Two other solutions gave values 36 and 39 at 19 and 24 hours respectively. Other curves were taken, particularly during the initial hour, but figure 4 is the most uniform. In some cases it happened that at a particular time the wave-length curve showed discontinuities, the points in every case showing a jump upwards though the subsequent rate of fall was unchanged. This may quite well have been due to an outside disturbance which would tend to break up any surface structure that was responsible for the lowering of surface tension.

(ii) *Solutions of cetyl pyridinium bromide in N/20 sodium bromide.* It was shown by Dr Adam that if the cetyl pyridinium bromide was dissolved in very dilute

electrolyte solution (e.g. sodium bromide) the rate of attainment of equilibrium was very much increased. In fact, after 3 or 4 seconds there was no further fall of surface tension. Accordingly, a solution of strength 0·003 per cent in $N/20$ sodium bromide was made up and used in the ripple apparatus.
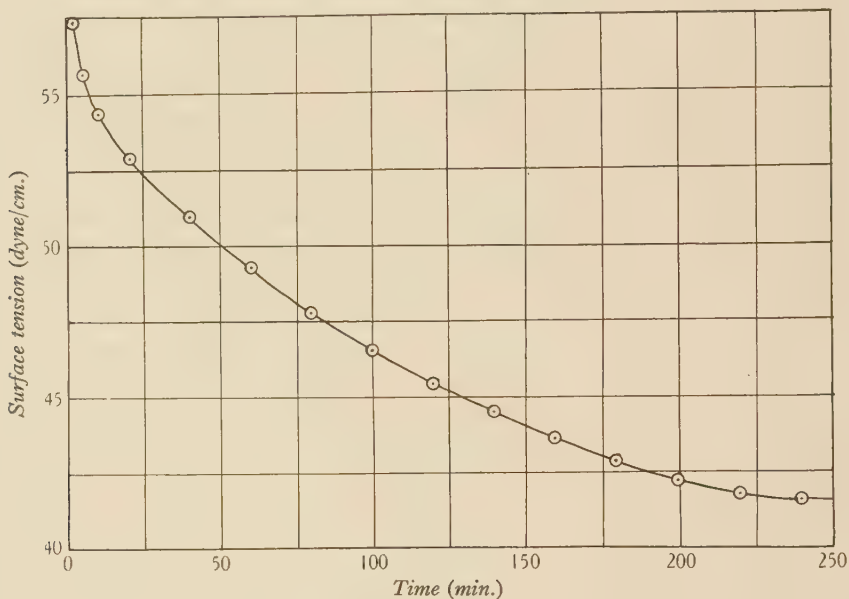
Figure 5. Change of surface tension with time. Aqueous solution of cetyl pyridinium bromide (0·003 per cent).

Since readings could not be taken until about $1\frac{1}{2}$ minutes after the formation of the surface, no fall of surface tension was observed, the first readings yielding almost the minimum value. The solution was supersaturated at air temperature and all readings were made at 30° C. The results are shown in table 1.

Table 1. Surface tensions of solutions of cetyl pyridinium bromide in $N/20$ sodium bromide

Frequency		Age of surface (min.)	Surface tension at 30° C. (dyne/cm.)
Same solution	50	5	31·6
		23	31·6
	100	5	30·9
		36	29·4
		57	30·4
		160	29·9
	200	7	30·3
		21	29·1
	300	7	29·6
		26	28·3
		40	28·4
Different solutions	200	—	30·6
	300	6	31·4
		33	30·4

§ 7. DISCUSSION OF RESULTS

(i) *Aqueous solutions.* The surface showed a steady approach to equilibrium, the final value of surface tension being in agreement with that found by Dr Adam, 36 dyne/cm. The rate of attainment of the final value seemed larger than that found in Dr Adam's static experiments, but no definite deduction should be made from this because he remarks that his curves were not particularly reproducible and because, further, if the rate of formation of the stable surface depends on the diffusion of molecules, it must be influenced by convection effects which are almost uncontrollable in the ripple apparatus. The results do, however, show conclusively that as far as the solution used is concerned, the passage of waves of capillary dimensions over the surface has no retarding effect upon the formation of the surface layer. The ripple method is not therefore comparable with the oscillating-jet method in this connexion.

(ii) *Solutions in electrolytes.* These results confirm the static nature of the ripple method, not only because the values which it gives for the surface tension of the solutions are the same as those given by static methods but also because there is no change of this value with the frequency of the waves. Results for different frequencies do show some diversity, but it is not sufficiently large or regular to suggest a serious effect of frequency on surface tension.

It must be remembered that apart from the small gravity term the value of the surface tension is determined by the cube of the wave-length, and since for a frequency of 300 the wave-length is 1·26 mm. the variations of surface tension shown in the table may well be due to experimental errors.

(iii) *Damping of waves due to surface layer.* It was noticed that the waves extended down the tank for a much shorter distance in the case of the cetyl pyridinium bromide solutions than in the case of pure water. Moreover, as the surface tension fell, damping increased. Thus it seems that the surface layer occasions considerably greater damping than does a pure water surface. The bulk viscosity of the solution presumably does not vary appreciably with time, whereas the surface structure does. This leads to the deduction that the damping of waves of capillary dimensions is a function of the surface structure.

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A METHOD OF MEASURING THE AMPLITUDE AND DAMPING OF RIPPLES*

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ABSTRACT. The optical arrangements of the author's ripple apparatus are such that determinations may be made of the amplitudes as well as of the wave-lengths of the ripples provided certain assumptions are made with regard to the wave-form. The fall of amplitude with distance of a maintained train of straight or circular waves is discussed, and the measurements taken show that the damping due to viscous forces causes a logarithmic falling off of amplitude. The damping factor is evaluated. In the case of circular waves the decrease of amplitude due to the spreading of the waves has to be taken into account. The amplitude of ripples which are satisfactorily observable is of the order of 2 or 3 × 10^{-5} cm.

§ 1. INTRODUCTION

IN the literature relating to the ripple method of measuring surface tension very slight reference is made to the amplitude of the ripples employed apart from the statement of the condition that Lord Kelvin's equation[1] holds only for small amplitudes. Both Dorsey[2] and Watson[3] mentioned the fact that when successive troughs or crests are used as cylindrical mirrors during the usual performance of the experiment, their focal lengths increased with their distance from the source owing to decreasing amplitude; but neither author passed from this to a computation of the amplitudes involved, although Watson actually published enough data to enable a rough calculation to be made. He hazarded an estimate of the amplitude of the waves at the source, basing it upon a measurement of the amplitude of the source itself.

Gorter and Seeder[4] made the first measurements of amplitude by causing a parallel beam of light to fall normally on the ripple surface and observing the maximum angle between reflected rays. This gave the maximum angle between tangents to the wave outline, and hence the amplitude of the waves. Measurements were taken when thin films were present on the surface, and a decrease of amplitude was observed when the films were compressed.

It is the purpose of the present paper to show how the amplitudes of the waves at any distance from their source may be calculated from measurements which it is possible to make during the use of a ripple apparatus such as that employed by Dorsey, Watson or the present writer[5].

§ 2. THEORY OF EXPERIMENT

The apparatus previously described [5] causes a parallel beam of light to fall vertically upon the liquid surface carrying the ripples. Focal lines are formed at the foci of the cylindrical mirrors comprising the troughs and crests. In the case of the troughs the images are real and are situated above the surface while those due to the crests are virtual and below it. In both cases the images move farther from the surface as the waves travel away from the source and their amplitude decreases.

Following Lord Kelvin, let us assume that the straight ripple may be represented by

$y, a, \lambda,$
x, v, t

$$y = a \sin \frac{2\pi}{\lambda} (x - vt) \qquad \ldots\ldots(1),$$

and further that the regions of maximum curvature are responsible for the production of the images. Here no account is taken of damping, since in the following analysis it is only necessary that the amplitude (a) should be constant over a distance of half a wave-length.

We obtain from equation (1) the relationship that at any instant

$$\frac{d^2y}{dx^2} = -\frac{4\pi^2 y}{\lambda^2} \qquad \ldots\ldots(2),$$

R

and, since the amplitude is always small compared with the wave-length, the radius of curvature R at a trough or crest will be numerically equal to the reciprocal of the maximum value of d^2y/dx^2, thus

$$R = \frac{\lambda^2}{4\pi^2 a} \qquad \ldots\ldots(3).$$

f

The focal length f of the mirror corresponding to this radius will be given by

$$f = \tfrac{1}{2}R \qquad \ldots\ldots(4),$$

so that if f and λ are measured the amplitude may be calculated from equations (3) and (4), thus

$$a = \frac{\lambda^2}{8\pi^2 f} \qquad \ldots\ldots(5)*.$$

In what follows we are concerned more with rate of decay than with precise values of amplitude and it is, therefore, only necessary to remember that

$$a = \frac{B\lambda^2}{f} \qquad \ldots\ldots(6),$$

B

where B is a number.

By a well-known result in hydrodynamics we know that in the case of gravity waves the kinetic and potential energies per wave-length are equal, their sum being equal to $\tfrac{1}{2}g\lambda a^2\rho$ per unit horizontal length perpendicular to the direction of propagation. The effect of surface tension may be allowed for by supposing that g is

g, ρ

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* When amplitudes are small it might be permissible to treat the half-wave outline between two consecutive points of zero displacement as an arc of a circle and not as a sine wave. This leads to the result $a = \lambda^2/64f$.

increased by the amount $4\pi^2 T/\rho\lambda^2$, which gives the total energy E per wave-length of ripples thus

$$E=\frac{a^2\rho\lambda}{2}\left\{g+\frac{4\pi^2 T}{\rho\lambda^2}\right\} \qquad \text{......(7)},$$

or simply

$$E=\pi a^2\rho v^2 \qquad \text{......(8)}.$$

Hence for a given liquid and a fixed frequency we can write

$$E=Ca^2 \qquad \text{......(9)},$$

where C is a constant.

In the case of straight ripples originating from an infinitely long dipper the action of viscous forces is the only factor influencing damping, which if assumed to be exponential may be represented by

$$E=E_0 e^{-Kx} \qquad \text{......(10)},$$

where K is a damping factor applying to decay of energy with distance x from the dipper, and E_0 is the energy at the dipper. Remembering equation (9) and putting $k=\frac{1}{2}K$ we may connect the amplitude at any point x with that at (but not of) the dipper by the equation

$$a=a_0 e^{-kx} \qquad \text{......(11)}.$$

Combining equations (11) and (5) we have

$$\frac{1}{f}=\frac{1}{f_0}.e^{-kx} \qquad \text{......(12)},$$

where f_0 is the focal length of the mirror whose amplitude is a_0. When natural logarithms are taken, equation (12) gives

$$\log f=kx+\log f_0 \qquad \text{......(13)}.$$

Thus if the foregoing analysis is correct we should expect a graph of $\log f$ against x to be a straight line and the slope of the line will give us k.

With circular ripples we should expect a falling off of amplitude on account of the spreading of the waves, apart from the effect of viscous forces. The total energy associated with one complete wave of radius x will be $2\pi xE$ since E represents the energy per unit length. If the fluid had no viscosity this expression would be constant for all values of x. The effect of viscosity may be allowed for by writing

$$2\pi xE=De^{-Kx} \qquad \text{......(14)},$$

where D is a constant representing the total energy at $x=0$.

Combining equation (14) with equations (9) and (5) and taking logarithms we obtain

$$\log\left(\frac{\sqrt{x}}{f}\right)=-kx+F \qquad \text{......(15)},$$

where F is a constant.

Thus we should expect a graph of $\log\left(\frac{\sqrt{x}}{f}\right)$ to be a straight line and the slope should give us the value of k. For the same liquid and frequency the value should be equal to that given by equation (13).

In the observations which follow, logarithms have been taken to the base 10 and consequently the values of the slopes must be multiplied by 2·303 for the purpose of evaluating k.

(Margin notes: T, E, C, K, x, E_0, k, a_0, f_0, D, F)

§ 3. METHOD OF EXPERIMENT

The writer's ripple apparatus which is fully described in the previous paper was modified only to the extent of replacing the cathetometer telescope by one of another type which could be focused by rotating a knob. To this was attached a circular scale of arbitrary divisions, and a fixed pointer was placed above it. A small piece of squared paper was clamped at various known distances above the surface of the liquid in the tank and the image of the paper was focused in the telescope. Observations were taken which enabled a curve to be drawn connecting readings of the focusing-knob with the distance above the liquid surface of the point upon which the telescope was focused.

Straight ripples having been established, the experiment consisted in bringing into focus the lines seen in the telescope at various points along the length of the tank. It was not necessary to know the actual position of the origin of the ripples in order to obtain a value of k, but no measurements were taken at points nearer to the origin than 3 cm., because the surface became curved by capillarity within this region. The middle portions of the lines in the centre of the field were focused. Usually the telescope was moved until the cross-wire was situated centrally between two adjacent lines. The cathetometer vernier gave the reading against which the logarithm of the focal length of the ripples was plotted.

It being established that apart from the experimental error the graph of $\log f$ against x was a straight line, it was decided to take measurements designed for the direct calculation of k. Two points were chosen at a convenient distance apart, and settings of the telescope were made first at one point and then at the other. Any change of amplitude common to the whole surface, such as might be due to contamination or a change in the amplitude of the valve oscillator, would result in a change in the readings at both points. If such changes were not large the mean reading of the focusing-scale at each point was taken and k was deduced by dividing the difference between the logarithms of f at the two points by the distance between them.

Actually the amplitude of the oscillator was checked by allowing part of the band of light produced by the vibrating mirror to fall on a ground-glass screen and observing the positions of the extremities. Very little variation of amplitude was found. Another pair of points was selected and k was calculated for a second range of distance. A few experiments were made with circular ripples produced by a glass style attached to the end of the dipper reed. In this case since $\log (\sqrt{x}/f)$ enters into the calculations, x must be measured from the origin of the ripples. The experiment was conducted in the same way as for straight waves.

§ 4. PRELIMINARY RESULTS

Straight waves. Figure 1 shows results for water at 20° C., the frequency of the ripples being 300 c./sec. In taking the readings four or five settings of the focusing-knob of the telescope were made and the mean setting for each distance was taken. Before the telescope was moved to the next position the surface was swept. As is

indicated in the figure, settings were made first as the telescope was being moved away from the dipper and then as it was being brought back. The line drawn gives more weight to the points taken during the first half of the experiment than to the remaining points. The slope is 0·191 per cm.

Results of measurements made between two fixed points in the wave train are shown in table 1. The first two rows represent readings taken in succession. The third and fourth were recorded on different occasions.
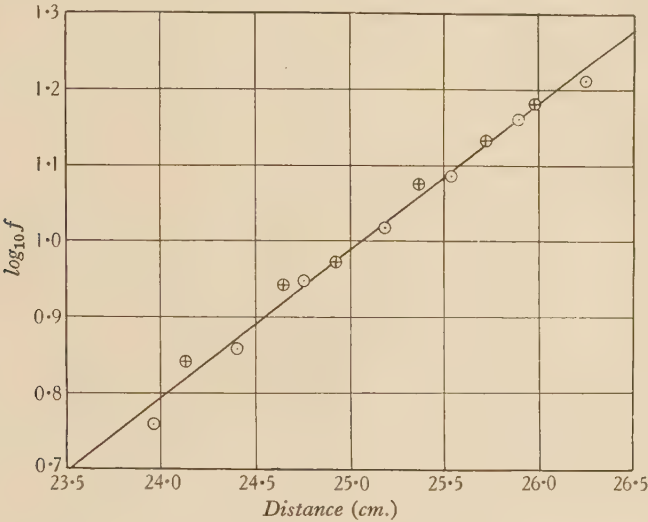
Figure 1. Damping of straight ripples; ⊙ distance increasing, ⊕ distance decreasing.

Table 1. Damping of ripples on distilled water at 20° C. Frequency of ripples, 300 c./sec.; wave-length, 0·172 cm.

Position of telescope (cm.)		Mean readings of focusing-knob		Focal lengths of ripples (cm.)		Slope, $\dfrac{\log_{10} f_p - \log_{10} f_q}{P - Q}$
P	Q	p	q	f_p	f_q	
25·75	23·78	1553	1062	14·50	6·02	0·194
25·23	24·28	1397	1154	11·98	7·78	0·197
25·74	23·70	1577	1060	14·90	6·02	0·194
23·39	21·68	1579	1103	14·91	6·83	0·198
					Mean	0·196

Each of the focusing-knob readings is the mean of at least ten observations. The focal lengths were, of course, read off from the calibration curve. From the mean value of the slope we find that $k = 0.451$ per cm. For any particular value of the focal length of the ripples the amplitude is obtained immediately from equation (5); for instance, taking $f = 14.50$ cm. from table 1 we obtain $a = 2.6 \times 10^{-5}$ cm.

It is a matter of some surprise that ripples which can easily be observed in the usual experiments designed for that purpose should turn out to have such small amplitudes.

Circular waves. A few attempts were made to test equation (15), which should give a straight-line plot. Figure 2 represents a set of results. Again the temperature of the water was 20° C. and the frequency was 300 c./sec. The points seem
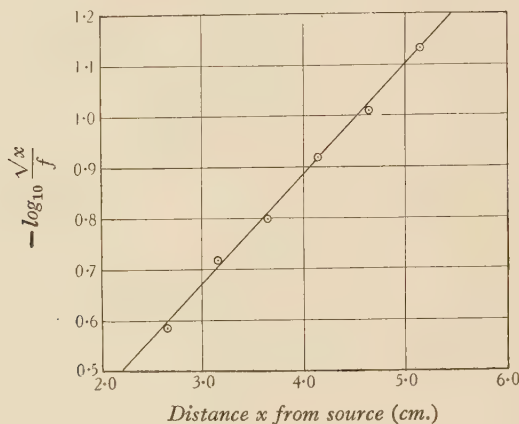
Figure 2. Damping of circular ripples.

to lie on a straight line the slope of which is 0·212. This is not in very good agreement with the slope given by straight waves. More careful experiments with circular waves are required before any definite statement can be made.*

§ 5. ACKNOWLEDGMENT

The author desires to thank Prof. E. N. da C. Andrade, F.R.S., for having granted facilities for the carrying out of this work, and also for the interest in it which he has displayed during its progress.

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* It is quite possible that, owing to the greater rate of decay of amplitude in the case of circular ripples, the assumption that amplitude is constant over a distance of half a wave-length is no longer permissible.

DISCUSSION OF THE PRECEDING TWO PAPERS

Dr G. F. C. Searle. An interference method of measuring the wave-length of the ripples may be possible. If a horizontal glass plate were placed close to the surface of the liquid, the interference bands seen with suitably interrupted green mercury light would be arranged in groups. The distance between the centres of successive groups could be measured.

Author's reply. Dr Searle's suggestion is ingenious but the experiment might, I think, be difficult, and the accuracy of the wave-length measurements would not exceed that obtained by the direct method. The amplitude of ripples used in surface-tension measurements is of the same order of magnitude as the wave-length of light, so that the fringes would be broad and few in number. The experiment might, however, be used as a means of checking measurements of amplitude.

Dr E. Tyler. The smallness of the fraction of the cycle during which the illumination persists suggests that the method is unsuitable for demonstration by projection on a screen. For projection purposes I have successfully employed a dual-reed synchronous arrangement in which one reed renders the light emitted by a 36-watt autolamp intermittent at mains frequency (50 cycles/sec.) before it falls on the ripple tank. This is made possible by bending the end of the maintained reed so that its end, when at rest, is coincident with the horizontal image of the lamp filament. The second reed, in series with the interrupter, is also operated from the same a.-c. supply, and carries the dippers; thus synchronization between the ripple formation and the intermittent illumination is automatically established.

It might therefore be an advantage to the author if he replaced the Pointolite lamp P', the slit S_1, and mirror M_1, by an autolamp reed interrupting device in the manner mentioned above. A slot may be fixed to the end of this reed if shorter times of flashing are required, but the question of width of slot is one of compromise with the amount of light available. The time of exposure required for photography is reduced considerably and there is no need to use ultra-fast plates. With Imperial Process Eclipse Plates, speed 650, successful photographs can be obtained with exposures ranging from 1 to 4 seconds. As regards measurement of the photographs, has the author tried photographing a graduated glass scale placed in the plane of the ripple surface, the image obtained being used for calibration purposes in evaluating λ?

A photocell traversing device might also prove useful in this connexion. The adoption of modern electric discharge tubes as stroboscopic illuminants may be recommended in certain cases for ripple-tank work. Both the Edgerton stroboscope* and B.T.H. stroboscopic discharge tube† serve admirably in this respect. I hope to demonstrate shortly some of the effects obtained with various types of discharge-lamp sources. Finally the author refers to the more thorough exploration into the

* Pamphlet, General Radio Co., Catalogue G, part 2, pp. 178–179 (March 1934), Cambridge, Mass.
† *Physical Society Exhibition Catalogue*, p. 165 (Jan. 1936).

possibilities of the vibrating reed idea for producing an intermittent beam of light. May I add that in this direction the use of the autolamp reed arrangement for producing a beam of intermittent light at a standardized frequency (50 c./sec.) has been successfully employed by me in the following ways, reference to which is made in a recent article.* (*a*) For the stroboscopic measurement of speed. (*b*) As a time base in a photographic falling-plate method for determining *g*. (*c*) As a stroboscopic illuminant in the study of falling liquid drops. (*d*) For visual demonstration of the nature of a transversal wave. If intermittent light is allowed to pass through a narrow aperture in a fixed screen and the resulting beam then falls on a piece of cardboard capable of being vibrated by hand, an apparently stationary transversal luminous wave system will be observed by persistence of vision between the screen and the cardboard.

AUTHOR'S reply. Some years ago I set up the ripple experiment for measuring surface tension using a reed carrying a slit, the line source being a stationary illuminated slit. In this I was following closely the technique described separately by Dorsey, Watson and Barnett. Dr Tyler uses a straight filament instead of an illuminated slit. In suggesting that the method described in the present paper was a novel one, I was referring to the use of the vibrating mirror rather than to that of the reed, which is, of course, a cheap and simple substitute for a tuning fork.

Whatever system of observation is used the fraction of the wave-length through which the ripple moves while it is being observed is that fraction of the time period for which illumination takes place. In my apparatus this may be reduced almost indefinitely. To achieve the same result with a vibrating slit requires such narrowness of the slit that optical adjustment may become difficult.

It should be remembered that in surface-tension work ripples of small amplitude are required, whilst those used in demonstrating the properties of wave-motion usually have much larger amplitudes. This is shown by the fact that the camera or other optical apparatus used in demonstrations requires to be focused on the liquid surface, whereas with ripples suitable for surface-tension measurements the images observed are at distances up to 30 cm. from the surface. Figure 2 in the first paper shows lines of considerable breadth, although the time of illumination was suitably reduced. This is because the ripples were shallow. With larger amplitudes cross-wire sharpness is easily obtained.

Dr Tyler's question regarding the measurement of wave-lengths by photography raises an interesting point which I once investigated. I tried for some time to measure wave-lengths with the help of a telescope having a fixed central cross wire and, parallel to it, another one which could be moved by means of a graduated screw which was calibrated by placing a scale in the plane of the images observed, and not in the surface as Dr Tyler suggests. The results were considerably in error because whereas the central image in the plane of the cross wires was formed by reflection from a crest, the adjacent one was formed by reflection from a point

* Tyler. *School Sci. Rev.*, pp. 238–239 (Dec. 1935).

slightly displaced from the crest of the next wave. This must be so since both reflected rays cross at the optical centre of the telescope objective. A correction was attempted and was partially successful, but this method of observation was finally abandoned. This kind of error is, it seems to me, necessarily present in any experiment involving the photography of ripples with a camera.

The new stroboscopic discharge lamps are remarkable and will probably be very useful in demonstrations of ripples, particularly if they can be operated independently of the a.-c. supply so that by slightly changing their frequency, while that of the ripples remains constant, the student may be enabled to distinguish between progressive waves and standing patterns. I shall look forward with interest to Dr Tyler's demonstrations.

THE β-RAY SPECTRA OF SOME INDUCED RADIO-ACTIVE ELEMENTS RESULTING FROM NEUTRON BOMBARDMENT

By R. NAIDU, D.Sc. and R. E. SIDAY, B.Sc.

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ABSTRACT. A preliminary determination of the energies of the end points of the β-ray spectra of some unstable elements produced artificially by neutron bombardment was made, a cloud chamber being used. Although these energies are for the most part considerably higher than those encountered in natural β activity, they fit the Sargent curves fairly well.

Between 4 and 10 per cent of the tracks observed are positively curved. No evidence is available to explain their origin unambiguously.

§1. INTRODUCTION: THE SCOPE OF THE EXPERIMENTS

THE periods of the β-ray disintegration of many new elements resulting from neutron bombardment have been determined recently by Fermi and his collaborators[1], and by others. The discovery of these new unstable nuclei has greatly extended the scope for investigations relevant to the new theories of β-ray decay; so we have used the method of the cloud chamber to find the sign of the charge of the particles in some cases where this was not previously known and to make a preliminary determination of the upper limits of the β-ray spectra of some of the more active of these elements.

In principle, this method gives immediately the complete disintegration curve for an element. However, with the radon sources available—about 100 millicuries—we could get a convenient number of tracks on each photograph by taking as much as possible of each element for activation. Consequently the sources used were *thick*, and so the shapes of our curves below the maxima are completely wrong, and the maxima themselves are probably slightly displaced.

§2. EXPERIMENTAL DETAILS

The automatic cloud chamber used is similar in design to one recently described by C. T. R. Wilson[2]. A rubber diaphragm, acting as a piston, moves below a fine wire gauze which constitutes the floor of the chamber. The expansion was obtained by suddenly evacuating the space below the diaphragm, the required volume-ratio being secured by limiting the final position of the diaphragm by means of an adjustable disc. In this way we could use the automatic mechanism devised by Blackett[3] for Wilson's earlier form of cloud chamber.

A magnetic field of 1000 gauss, uniform to 1 per cent over the illuminated part of the chamber, was provided by water-cooled solenoids. The tracks were about 5 cm. long. The method of illumination used—that of a flash from a mercury lamp activated by a current from a transformer—has been described by Blackett[4]. A pair of photographs was taken at each flash by cameras inclined at 90 and 75° to the plane of the chamber.

A cylindrical pot, of diameter 1·5 cm., made of very thin metal foil was let in axially through the top of the chamber. It was made long enough to reach the gauze so that the distortion of tracks due to the swirls that it is liable to produce in the gas would be minimized.

The elements were always prepared, either as foils or as powders wrapped in cellophane, in cylindrical form to fit nicely inside the pot. They were activated by the neutrons from radon-beryllium sources about 100 millicuries strong, those showing the "water effect" being activated in a cavity in a large paraffin wax block. After sufficient time they were transferred to the pot, and expansions were made while the activity persisted.

§3. RESULTS

Of the tracks obtained, those showing small angular deviations, together with all short tracks, were carefully rejected according to a definite criterion such as was used by Champion[5]. Then the strongly curved tracks were measured by fitting to them circular arcs of known radii photographed on slides, while for those less curved, the traversing-microscope method described by Anderson[6] was adopted.

The results of these measurements are shown in the form of block diagrams in figure 1. From the smooth curves drawn through these the end points have been very roughly determined, and are collected in table 1. However, the determination of the end points in this way is largely arbitrary.

Table 1

Element	Isotopes	Half-period studied	Upper limit of β-ray spectrum
9 F	19	9 S.	$7 \pm 2 \times 10^6$ eV.
14 Si	28, 29, 30	2·3 m.	$4 \pm 0·5 \times 10^6$
45 Rh	103	44 S.	$3·6 \pm 0·5 \times 10^6$
47 Ag	107, 109	$\begin{cases} 23 \text{ S.} \\ 2·3 \text{ m.} \end{cases}$	$3·8 \pm 0·5 \times 10^6$ $2·8 \pm 0·4 \times 10^6$
63 Eu	151, 153	9·2 h.	$2·6 \pm 0·4 \times 10^6$
66 Dy	161, 162 163, 164	2·5 h.	$1·9 \pm 0·2 \times 10^6$

The effect of errors in the curvature-measurement is to increase, in general, the number of tracks in the higher-energy groups, and to produce extra blocks beyond the true end points. By far the most serious of these errors is that arising from the distortion of tracks due to swirls in the gas of the chamber; these may completely falsify the estimation of high energies. The curvature of a 10-million-volt track—e.g. one in the middle of the tail extending beyond the end point of the

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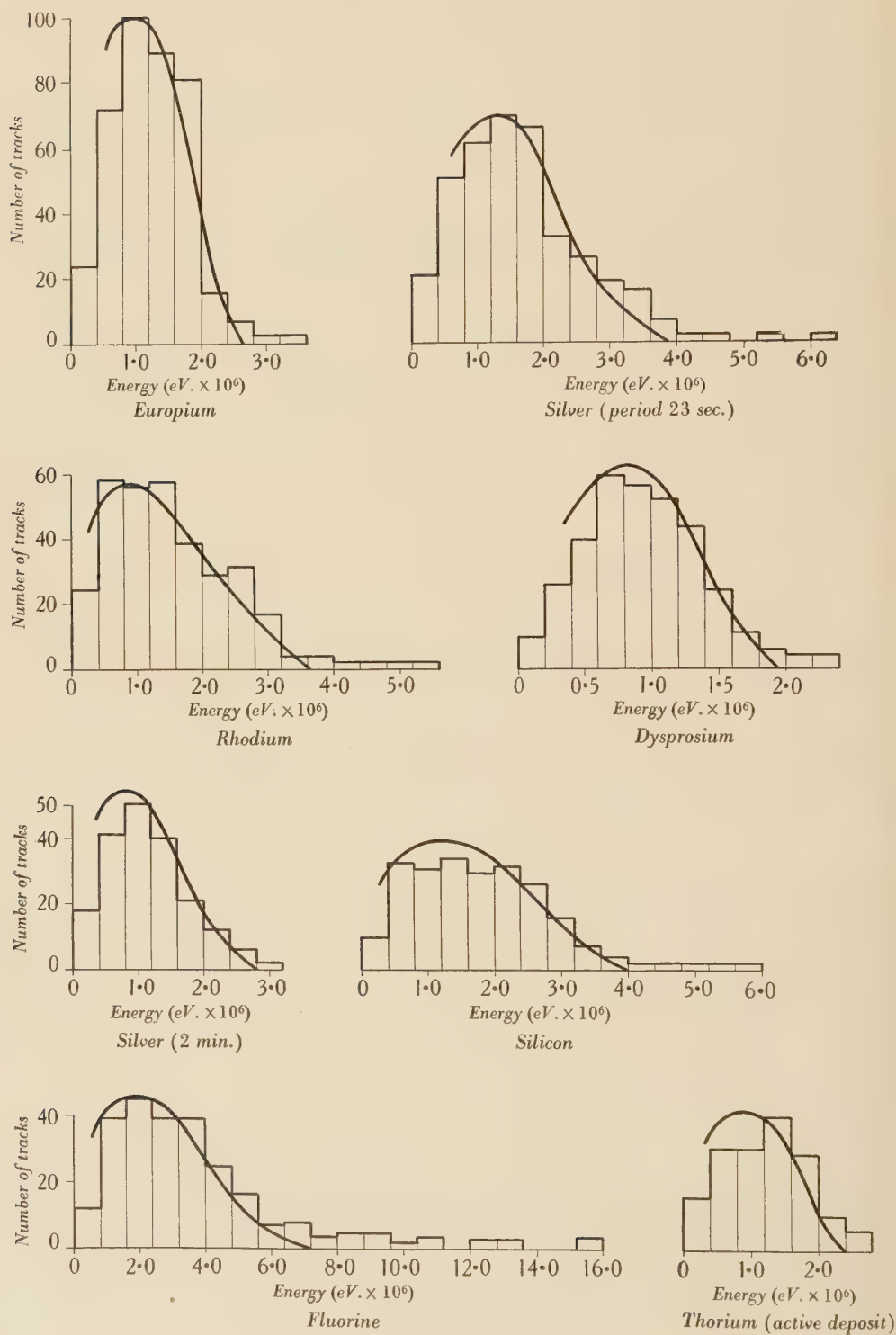
Figure 1.

spectrum of activated fluorine—is about 0·6 in arbitrary units. An analysis of β-ray tracks taken without a magnetic field showed that, as a result of this swirl distortion, our curvature-measurements were liable to an error of 0·2 units, or over 30 per cent for this case. We estimate the total probable error from all causes as being nearer 40 per cent. For the spectra of activated silicon and silver it is about 15 per cent. The long tails on the high-energy side of the spectra of these elements may then be entirely spurious, being due to this effect. Although some of the tracks may truly correspond to high-energy particles produced by the γ rays of unknown energy known to accompany the disintegrations, the end points obtained from the smooth curves will still have this uncertainty. These views are confirmed by an analysis of some photographs of the β rays from a source of thorium active
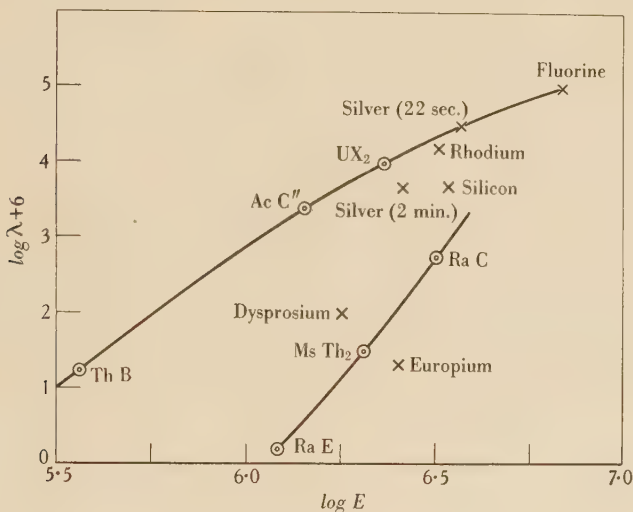
Figure 2.

deposit. The continuous spectrum has an end point of 2·2 million volts, and there is also a strong γ ray producing photo-electrons up to 2·63 million volts. We found a tail extending to 2·8 million volts, and an upper limit at 2·3 million volts; the probable error was expected to be about 10 per cent. The probable errors given in table 1 are determined from these considerations. That associated with the spectrum of activated fluorine is so large as to make the determination for this element practically worthless; further experiments with improved technique will be undertaken.

In figure 2 the logarithms of the end points are plotted against the logarithms of the corresponding decay constants to form Sargent curves[7]. The points seem to fall as well as might be expected about one or other of the two curves, especially as the end points given above are probably too high. The accuracy of the determination of the spectra is insufficient to justify any more elaborate comparison with theory.

§4. POSITRON TRACKS

For every element studied we have always found a few per cent of the tracks apparently coming from the source, but with positive curvature. By a rough estimation of the retrograde electron tracks from the walls of the cloud chamber one might account for a fraction of the positively curved tracks observed. But in some cases, e.g. europium, these apparent positrons constitute as much as 10 per cent of the measured tracks, and it is difficult to give an unambiguous answer to the question of the origin of these tracks. Further experiments are in progress to elucidate this question.

No certain case of the production by an electron of a positron-electron pair in the gas has been observed. In the course of this work we have photographed about 100 metres of track of β rays greater in energy than one million electron-volts and have observed only one doubtful case. This result suggests that for oxygen the upper limit of the average cross-section for this process of positron production by β rays, if it takes place at all, is less than 10^{-23} cm². Skobelzyn [8] reports several cases of such production of positrons and estimates the cross-section to be about 10^{-23} cm².

§5. ACKNOWLEDGMENTS

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REVIEWS OF BOOKS

A Source Book in Physics, by WILLIAM FRANCIS MAGIE. Pp. xiv + 620. (McGraw-Hill Publishing Co., Ltd.) 30s. net.

The volume under review is a valuable contribution to the history of physics. It attempts to bring before the reader a picture of the outstanding events in the progress of science by supplying quotations selected from the publications of the pioneers when they announced their discoveries to the world. It will prove of great assistance to the mature student and the teacher as a reference work. Furthermore it can be dipped into with the assurance that it is never dull.

Opening the book at random one finds for example a quotation from Cailletet who, whilst directing his father's foundry at Châtillon-sur-Seine, carried out the relatively simple experiments which proved that oxygen could be liquefied. These experiments preceded by only a few weeks the more elaborate ones at Geneva of Pictet, whose discovery was communicated to the French Academy in the despatch " Oxygen liquefied to-day under 320 atmospheres and 140° of cold by sulphurous acid and carbon dioxide in succession ".

Fizeau at the age of 30 describes the first successful attempt to measure the velocity of light by means of observations that do not involve astronomical constants. He says " These first trials furnished a value of the velocity of light only a little different from that which is accepted by astronomers. The mean value determined from twenty-eight observations which have been made up to this time is 70948 leagues of 25 to the degree ".

To anyone wishing to become acquainted with the style of writing of Galileo, Newton, Lagrange, Cavendish, Ampère, Davy, Joule, Kelvin, Maxwell, Fizeau, Faraday, Curie, Crookes, or Roentgen, to mention but a few, the volume affords the means with the least amount of trouble.

The author of the work is to be congratulated on having made a very useful contribution to the literature of physics.   E. G.

Praktische Physik, by F. KOHLRAUSCH. Pp. x + 958. (Teubner, Leipzig.) RM. 24, in England.

Kohlrausch's *Praktische Physik* is an old friend whose first acquaintance some of us made in our student days. Successive editions of this useful handbook have faithfully mirrored the changes which are taking place with time in the domain of practical physics. Many an expert has contributed his quota to build the present imposing structure based on the foundations so truly laid by Kohlrausch single-handed in 1872. The general editor of the volume under review is Dr Henning and several of his collaborators are members of the staff of the Physikalische-Technische Reichsanstalt.

Written by men whose daily work is to make measurements under conditions where accuracy and convenience counts for more than mere novelty of method, this volume is characterised by discrimination and judgment that have been exercised in the selection and presentation of the material. It is a most valuable work of reference and the contributors are to be congratulated on the production of such a comprehensive treatise.   E. G.

Organic Solvents: Physical Constants and Methods of Purification, by ARNOLD
   WEISSBERGER and ERICH PROSKAUER. Translated from the German manuscript
   by RANDAL G. A. NEW. Pp. 212, including subject index and bibliography with
   60 references to books and 1406 to periodicals. (Oxford University Press.) 15s.

A self-evident fact that has not always been acted on in the past is that the reliability
of a number purporting to represent a physical constant of an organic liquid depends not
only on the accuracy with which the measurement is made but also on the purity of the
liquid. The determination to many decimal places of a physical constant of a *pure* organic
solvent is a great achievement and of permanent value, but such a number is a double-
faced villain if water or one or more of a host of possible organic impurities contaminate
the liquid on which the experiment is made. The authors of this book rightly point out
that there is an "ever increasing demand for variety and purity in organic solvents", and
wide-spread gratitude will be felt towards them, one an organic chemist and the other a
physical chemist, for compiling this compact treatise, which possesses an admirable
structure. The general appearance of the pages is attractive and the descriptive passages
have been lucidly translated.

The purification and physical constants of one hundred and fifty-seven organic liquids
are discussed. These liquids are classified as hydrocarbons (aliphatic saturated, aromatic,
aliphatic unsaturated), compounds with one type of characteristic atom or group (hydroxy-
compound, esters, amines, etc.), and compounds with more than one type of characteristic
atom or group (esters of hydroxyacids, etc.) and an intensive search has been made for the
best values available of the following physical constants: molecular weight, density,
melting- or freezing-point, boiling-point, vapour pressure, latent heat of evaporation,
specific heat, cryoscopic constant, ebullioscopic constant, viscosity, electrical conductivity,
dielectric constant, dipole moment, refractive index, ultra-violet absorption, infra-red
absorption, and flash point. Clearly there are very few physicists who will not find such a
book as this useful at some time or other, and it should certainly be near at hand when
organic liquids are being used in quantitative experiments.   T. M.